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SIR DAVID BREWSTER, K.H. LL.D. F.R.S. L. & E. &c.

RICHARD TAYLOR, F.S.A. L.S. G.S. Astr. S. &c.

AND

RICHARD PHILLIPS, F.R.S. L. & E. F.G.S. &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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ALERE FLAMMAM

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ERRATA.

Page 91, line 17, *dele* By continued effusions of distilled water.
—— 92, — 21–22, *after* lime *insert* water: *after* boiled, *insert a full stop.*
—— 92, — 28, *after* mentioned *insert* (4. and 5.)
—— 93, — 27, *after* water *insert* it was as copper;
—— 94, — 27, *dele* (28.)
—— 95, — 14, *for* (32.) *read* (38.)
—— 191, — 6, for s —, read s +, *and for* α *read* a.
—— 191, — 13, for y^2—, read y^2+.

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[THIRD SERIES.]

JULY 1834.

I. *On Magnetic Attraction and Repulsion and on Electrical Action.* By ROBERT WERE FOX*.

HAVING observed, whilst I was using my dipping-needle with its magnetic deflector (noticed in No. 20, Third Series of the London and Edinburgh Philosophical Magazine: vol. iv. p. 81), that the ratio of the magnetic forces was not uniform, I was induced to enter upon a series of experiments for the purpose of investigating these forces.

Of the various methods I adopted, I think it may be sufficient for me to describe that only which seemed on the whole to be most satisfactory and decisive.

To the extremity of a horizontal beam, delicately balanced on knife-edges, a magnetic bar was attached, with its axis in a vertical direction, and a counterpoise was suspended from the other end of the beam. Immediately under the attached or suspended magnet, and in the same vertical line, another similar magnet was placed, in a glass tube, in which it could be moved freely up or down, a graduated scale having been fixed to the tube to mark the exact distance between the contiguous poles of the two magnets; and when the poles were brought very near each other, mica, paper, card-board, &c., were placed between them, 2, 4, or 8 pieces being pasted together to obtain the duplicate ratios of the distances. Minute weights were suspended to ascertain the force of attraction or

* Communicated by the Author, on the 9th of April last.

repulsion, as the case might be; and a small pin was fixed immediately above the suspended magnet, in order to insure the uniformity of its position, the weights being regulated accordingly.

I first attached to the beam a small cylindrical magnet $1\frac{1}{2}$ inch long and $\frac{1}{5}$th of an inch in diameter, the south end being downwards; a similar magnet was placed in the tube with its north end upwards; when the ratio of their attractive forces at different distances appeared to be as follows:

Distance.	Weight in Tenths of a Grain.	Distance.	Weight in Tenths of a Grain.
$1\frac{1}{3}$ inch ..	1·0	$\frac{1}{12}$ inch ..	101·
$\frac{2}{3}$ — ..	3·5	$\frac{1}{24}$ — ..	204·
$\frac{1}{3}$ — ..	13·	$\frac{1}{48}$ — ..	411·
$\frac{1}{6}$ — ..	42·5	$\frac{1}{96}$ — ..	821·

The same suspended magnet attracting a cylinder of soft iron, of the same dimensions, substituted for the magnet in the glass tube:

Distance.	Weight in Tenths of a Grain.	Distance.	Weight in Tenths of a Grain.
$\frac{2}{3}$ inch ..	·5	$\frac{1}{24}$ inch ..	46·
$\frac{1}{3}$ — ..	1·8	$\frac{1}{48}$ — ..	96·
$\frac{1}{6}$ — ..	7·	$\frac{1}{96}$ — ..	197·
$\frac{1}{12}$ — ..	22·		

The arrangement the same as the foregoing, except that the north pole of the second magnet was in contact with the lower end of the soft iron in the tube:

Distance.	Weight in Tenths of a Grain.	Distance.	Weight in Tenths of a Grain.
$\frac{2}{3}$ inch ..	1·0	$\frac{1}{24}$ inch ..	94·
$\frac{1}{3}$ — ..	4·	$\frac{1}{48}$ — ..	195·
$\frac{1}{6}$ — ..	12·5	$\frac{1}{96}$ — ..	386·
$\frac{1}{12}$ — ..	42·		

I afterwards substituted for the magnets above described two others, each 3 inches long and $\frac{1}{10}$th of an inch in diameter, when the following results were obtained:

The suspended magnet with its north end downwards:

Attracting.

Distance.	Weight in Tenths of a Grain.	Distance.	Weight in Tenths of a Grain.
1 inch ..	2·0	$\frac{1}{32}$ inch ..	211·0
$\frac{1}{2}$ — ..	7·5	$\frac{1}{64}$ — ..	432·
$\frac{1}{4}$ — ..	19·5	$\frac{1}{128}$ — ..	810·
$\frac{1}{8}$ — ..	48·	$\frac{1}{256}$ — ..	1590·
$\frac{1}{16}$ — ..	99·	in contact	12000·

Repelling,—the poles of the lower magnet being reversed:

Distance.	Weight in Tenths of a Grain.	Distance.	Weight in Tenths of a Grain.
1 inch ..	2·5	$\frac{1}{32}$ inch ..	9·
$\frac{1}{2}$ — ..	7·5	$\frac{1}{64}$ — ..	9·
$\frac{1}{4}$ — ..	14·5	$\frac{1}{128}$ — ..	9·
$\frac{1}{8}$ — ..	25·	$\frac{1}{256}$ — ..	9·
$\frac{1}{16}$ — ..	29·		

The arrangement the same as before, but the force of the lower magnet diminished:

Attracting.

Distance.	Weight in Tenths of a Grain.	Distance.	Weight in Tenths of a Grain.
$\frac{1}{2}$ inch ..	2·0	2 thin plates of mica	1320.
$\frac{1}{4}$ — ..	8·	4 Ditto do.	720·
$\frac{1}{8}$ — ..	20·5	An exceedingly thin film of mica which polarised light ..	9,660·
$\frac{1}{16}$ — ..	42·5		
$\frac{1}{32}$ — ..	85·		
$\frac{1}{64}$ — ..	175·	Ditto do. doubled	5,020·
$\frac{1}{128}$ — ..	390·	Surfaces of magnets in contact	10,080·
$\frac{1}{256}$ — ..	803·		
Tissue paper interposed	1140·	Edges only in contact	13,600·
1 thin plate of mica ..	2400·		

Repelling.

Distance.	Weight in Tenths of a Grain.	Became attractive	Weight.
$\frac{1}{2}$ inch ..	2·0	at $\frac{1}{32}$ inch ..	10·
$\frac{1}{4}$ — ..	4·	$\frac{1}{64}$ — ..	39·
$\frac{1}{8}$ — ..	5·	$\frac{1}{128}$ — ..	77·
$\frac{1}{16}$ — ..	2·	$\frac{1}{256}$ — ..	143·
		in contact ..	1223·

The above results, which are only a small portion of those I have obtained, may be regarded as tolerably near approximations to the truth under the circumstances of the case, and are, I think, sufficient to show that the *laws* of magnetic attraction and repulsion alter according to the distance of the magnets from each other, the force at small distances being in the simple inverse ratio of the distance, and when further separated, in the inverse ratio of the square of the distance. This change of ratio in the case of attraction, gradually took place at the distance of from $\frac{1}{8}$th to $\frac{1}{4}$th of an inch, and even at $\frac{1}{2}$ an inch from each other when I used larger magnets; and in the case of repulsion, the change in the law occurred at much greater distances, especially when the forces of the respective magnets were materially different.

When soft iron is attracted by a magnet, the law changes

at a smaller distance than when two magnets attract each other; but when soft iron is made to repel a magnet, by being in contact with the corresponding pole of another magnet, the law approximates to that of the simple inverse ratio, even at very considerable distances; indeed, in this case the law of the inverse ratio of the squares scarcely ever obtained at distances at which the magnetic action was very perceptible in the method which I adopted to ascertain these forces; and when they were brought near each other, the repulsive force rapidly diminished, and ultimately became attractive. This also occurred, although in a less degree, when the similar poles of two magnets acted upon each other, if they materially, or even moderately differed in intensity.

The attraction under these circumstances at the commencement seemed to be in the duplicate inverse ratio, and on the magnets being brought nearer together, in the simple inverse ratio, of the distance.

It may here be observed that the force of attraction was considerably greater when the *edges* only of the magnets were in contact, than when their whole surfaces were together, which is perhaps attributable, in part at least, to the contact being more complete in the former case than in the latter. Hence it seems that the small magnets I employed lost one half their force at the distance of about $\frac{1}{2000}$th of an inch, one quarter at $\frac{1}{1000}$th, &c.

It is well known that pulverized magnetic iron, or iron filings, will *diverge* at each pole of a magnet; and I found that on making two *dissimilar* poles approach each other, this divergency was not changed till they were brought within certain degrees of approximation, in fact, sufficiently near to produce the change in the law above mentioned: the divergency then diminished, till the fibres of powdered loadstone became parallel to each other, and to the axis of the attracting poles, and so they continued till the magnets were in actual contact (see figs. 1. and 2.). This experiment seems to afford distinct evidence of the change which the magnetic elements sustain, and sufficiently explains the immediate cause of the alteration in the law of the attractive force which has been noticed.

When *similar* poles are gradually made to approach each other, the divergency of the adhering particles of loadstone is sooner disturbed than in the case of attraction, it being more and more increased, till the ferruginous filaments are repelled at nearly right angles to the axis of the magnet; and if the magnets differ much in force, the filaments attached to the weaker one are so far driven back as to become nearly paral-

lel to its axis, the *ends* being actually *reversed* (see figs. 3 and 4.) Under these circumstances they attach themselves

Fig. 1.

Fig. 2.

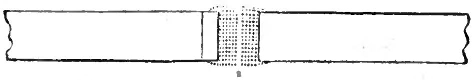


Fig. 3.

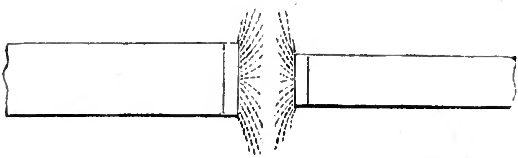
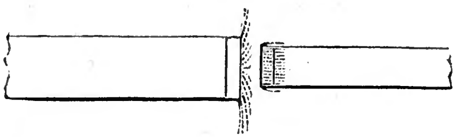


Fig. 4.



to the stronger magnet, and afford a very simple explanation of the cause of the rapid diminution of the repulsive force, and the change to attraction which takes place between magnets of unequal intensity whilst they remain in these relative positions. The appearances assumed by the particles of loadstone may also be exhibited by strewing them on paper, glass, &c., under which magnets have been previously fixed at proper distances from each other, and gently tapping the surface, till the arrangement is effected.

I am disposed to regard the magnetic curves around the magnet as *induced* towards its surface, and not thrown out from it, the magnetic elements without, having the same properties as those within the magnet, and the opposite poles in both cases alternating with each other throughout the whole series. Hence a magnetic circuit is formed, and the *tension* thereby produced between the opposite poles of a magnet may essentially contribute to maintain its intensity. This

tension, added to the reciprocal lateral repulsion of the terminating poles of the magnetic elements, must, I conceive, impart to the different series of them within a magnet, a curvilinear arrangement, the convex sides of the curves being turned towards its axis; and the very tenacious manner in which the particles of loadstone so long retain their divergency at a pole of a magnet, when strongly attracted by another magnet, is, I think, only to be explained by a reference to this reciprocal repulsion of the elementary poles, and the tension produced by the external magnetic curves.

That these magnetic curves extend to an extraordinary distance from a magnet, may be easily proved by their sensible action on a very minute and delicately suspended needle, which has been neutralized with respect to the directive force of the earth's magnetism. On using this test I found that a magnet, 3 inches long and $\frac{1}{10}$th of an inch in diameter, was invested with a magnetic sphere, (if I may use the expression,) which occupied considerably more than a *million times* its own bulk, ample allowance having been made for the space filled by the magnetic curves round the suspended needle itself.

This fact, taken in connexion with the rapid decrease of the force of a magnet at very minute distances, and the inappreciable change in the earth's magnetic intensity* at the greatest distances above and below its surface which have been examined, go far to show that the influence of the terrestrial magnetism may probably extend to a vast distance from our globe; and if the magnetic forces be common to the planetary system, the *remarkable uniformity* in the places of the *nodes* of most of the planets in relation to the *plane of the solar equator* may, perhaps, be referred to their agency.

I am unwilling to conclude this communication without remarking, that the fact I have stated relative to the particles of loadstone attached to the end of a magnet having their

* From numerous experiments which I have made at various times, in addition to what others have done, I have no doubt of the fact of there being very small apparent temporary fluctuations in the terrestrial magnetic intensity by day, and occasionally by night; although these changes may be due merely to *variation in the magnetic dip*. Among the *local* affections of the magnetic needle, I will mention one instance which my relative Arthur K. Barclay has communicated to me. In a tour of pleasure which he last year took through Italy, Sicily, Greece, Turkey, &c., he kindly made, at my request, many observations with a horizontal needle deflected by a magnet in a fixed relative position; and at the base of *Trizza*, or the *Cyclops' Rocks*, he observed a deflection of 24°, whereas at their *summit* it was 30°. The former consists of columnar basalt, and the latter of sandstone. My friend repeated these observations several times with the same results. At Messina, Catania, &c., the deflection of the needle was also 30°.

poles reversed on the approach of the similar end of another stronger magnet, seems calculated to throw light on the *opposite* electric relations acquired by two bodies when brought into mutual contact; for, assuming that the electric elements, like electrified bodies, possess opposite polarities both of attraction and repulsion, is it not reasonable to believe that, like the magnetic elements, those electric poles least tenaciously retained in one body, or most energetically opposed by similar poles in another body, will turn from them, and even be actually *inverted*, in order to adjust the balance of forces between the positive and negative electricity in the two bodies? This appeared to me to be a necessary consequence of magnetic repulsion, and the attraction acting on the opposite elementary poles, before I had ascertained the fact; and I think that the same argument will apply with nearly equal force to electric repulsion and attraction, and the cause of a very obscure phænomenon may, in this way, be simply explained.

Hence it seems to follow, that the *direction* of the elementary poles may be sufficient to determine the positive or negative character of electricity in all cases of electrical development; and supposing the mode of elementary action already noticed (viz. the reversing of the poles,) to prevail throughout a voltaic circuit, pulsation would be produced, and probably an actual *rotation* of the electric elements. Such rotation would account for the appearance of opposite currents; and upon the whole, it seems to me that this hypothesis is calculated to afford simple and satisfactory explanations of many of the phænomena which electricity presents,—especially its powerful effect on compound bodies, and the transfer of their constituents to the opposite electric poles, — whilst it does not demand from us the admission (in the case of voltaic electricity,) of continuous currents of immeasurable velocity, and transferring almost inconceivably large quantities of electricity from one body to another.

It may, perhaps, be well for me to state more fully my views of the manner in which simple electric elements, similar to, or *identical* with, the magnetic elements, may unite with matter, and exhibit electric effects in consequence of their natural relations being disturbed, my object being merely to offer some examples, without insisting upon them, for the purpose of showing that the elementary action and rotation I have suggested *may* enable us to explain many phænomena better than the commonly received hypothesis of electric "*currents*" in rapid circulation, such, for instance, as the *definite* action of electricity, which has been so clearly demonstrated by our first experimental philosopher.

If we suppose the positive electric poles to adhere to, or have an affinity for, the atoms of oxygen, and the negative poles for the atoms of hydrogen, the external or *acting* poles which surround these atoms will, on this hypothesis, possess opposite properties to the *adhering* poles, and impart to oxygen *negative*, and to hydrogen *positive* characters.

Between these strong opposite affinities, the force of electrical attraction or adhesion to the atoms of matter may be variously modified, according to the nature of the latter. Hence atoms may act on each other in consequence of their *electric properties*, and have these properties modified, and in some cases even reversed, by the inversion of the poles when acted upon by other atoms around which the electricity is more energetic and decided. Electricity excited in bodies by induction will illustrate this hypothesis; and the apparent anomalous chemical compounds which so often occur, such, for instance, as oxygen or hydrogen with the same simple elements, may serve as examples.

In the case of voltaic action the atomic electric affinities may be partially superseded, and an uniform direction simultaneously imparted to all the corresponding elementary poles in the circuit, an undulatory motion, or actual rotation of these elementary poles, being generated by their reaction.

Fig. 5. may illustrate how a rotatory motion of the electric elements might operate in producing decomposition.

Fig. 5.

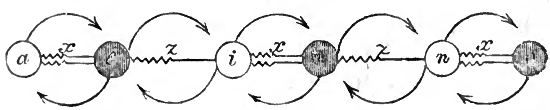
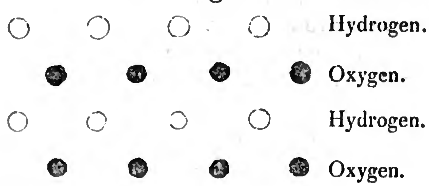


Fig. 6.



Let the shaded circles (fig. 5.) represent atoms of oxygen, and the unshaded ones those of hydrogen, included within the voltaic circuit; and let the straight lines denote the electric elements with their negative or *oxygenous* poles outwards, and the zigzag lines those having their positive or *hydrogenous* poles outwards. In this case, the electric elements $x\,x\,x$ are in the

natural order of their affinities with respect to the atoms *a e*, *i m*, and *n o*, whilst the electric elements at *z z*, are reversed with respect to *e i* and *m n*, the former atoms being made to approximate, and the latter to recede from each other, eventually giving rise to a semi-revolution of the atoms around their common axes as shown by the arrows.

The relative position of the different atoms in water and other fluids must, I think, have reference to their comparative weight, or downward tendency.

Fig. 6. may illustrate this idea, in which a series of atoms of oxygen are placed under a similar series of atoms of hydrogen; for they cannot, I conceive, under any circumstances, alternate *horizontally* with each other.

It is possible that the state of fluidity may be due to an excess of one of the electric polarities over the other, thereby preventing the atoms from approaching within certain distances of each other.

Now, if we again refer to fig. 5. the directions of the arrows exhibit a transfer of the hydrogen through the upper portion of the circle, and the oxygen through the lower; because in this case it is assumed that the decomposing surfaces are bounded above, by the atoms of oxygen, and below, by those of hydrogen. They cannot, however, in either case be readily carried beyond one half of a revolution, because they would then be respectively opposed by atoms similar to themselves; but the electric elements, on their quitting the atoms, will not be prevented by their polar affinities from completing their revolutions, and returning to their previous direction in readiness to renew the operation with other atoms.

The relative positions which I have supposed the different atoms in fluids to assume, may tend to impart a definite character to electrical action; and as it is not easy to imagine that the electric elements will move with an equable velocity during the whole of their revolution, if they are charged with atoms of matter during the performance of one half of it only, or that the time required to combine with, and to disengage themselves from the atoms will be precisely the same, it seems to follow as a probable consequence that voltaic electricity may act on magnets by virtue of an *excess* of force or influence in one direction over another.

In the case of thermo-electricity, likewise, the time required to disturb and restore the natural arrangements of the electric elements may materially differ.

On the hypothesis that the magnetic elements have their opposite poles alternating with each other throughout the curves which unite the two ends of a magnet, it is not difficult

to understand that if these curves are interrupted and broken, and the magnetic elements coerced from their natural position by the approach of an electrical conductor, such a reciprocal action must occur between the magnet and conductor as will cause them, if freely suspended, to assume some other relative positions, the uniformity of which may be governed by the arrangement of the magnetic elements with respect to each other. The opposite poles of the latter may overlap each other, for instance, in one uniform manner so as to determine them towards a tangential position with respect to the axis of a magnet when acted upon by an electrical conductor: the *spiral* form, and perhaps some other forms, might impart to them this tendency. The same reasons may explain why a magnet does not produce electricity in a metallic wire when moved in the direction of their common axes, as the opposite poles of a magnet, in this case, have a tendency to counteract each other's influence.

Many phænomena in electro-magnetism seem to countenance the probability of a rotation of the electric elements, such as the rotation of mercury, water, &c., when under the joint influence of electricity and magnetism. There is also a mechanical fact, which I published some time ago in the Journal of the Royal Institution, and which, perhaps, may be considered as favouring the opinion of the rotatory motion, or rather tendency, even of common electricity, viz. that when a needle or any sharp-pointed instrument rapidly revolving round its own axis is made to pierce a card, a bur is raised *on each side* of the card precisely similar to the effects produced by an electric discharge*. Moreover, the analogies afforded by the apparent undulatory motion of light and sound, seem to favour the opinion that a corresponding action exists in the case of electricity.

Note.—Since I have completed the experiments above referred to, and, indeed, written the greater part of this paper, a friend of mine has sent me W. S. Harris's paper on the laws

* I do not mean to use this mechanical illustration as an argument in favour of a *complete* elementary rotation in the case of an electric discharge from a Leyden jar, for a *semirevolution* of the electric elements might, perhaps, suffice to produce the *burs*; and if we assume that the *opposite* electric poles attract each other on either side of the glass, such a *partial* rotation, or mere undulation of the elements in the series or circuit, when a conductor is applied, seems to be all that is required to restore the natural arrangement and equilibrium of forces. The burs might possibly, however, be produced by the meeting of the opposite undulations at the card. The same explanation may be given of the common electric spark, under ordinary circumstances, as of the electrical discharge from a Leyden jar: thus presenting a marked difference of action from what I have supposed to exist in the cases of voltaic, and thermo-electricity.

of magnetic forces, inserted in the Transactions of the Royal Society of Edinburgh, and I find that this gentleman has noticed a change in the law from the inverse ratio of the square to the simple inverse ratio of the distance, when two magnets were brought near each other; but the greatest degree of approximation he has given is $\frac{1}{5}$th of an inch. The magnets he used were much larger than mine.

II. *On the Falls of Niagara; with some Observations on the distinct Evidence which they bear to the Geological Character of the North American Plains.* *By* G. Fairholme, *Esq.**

IT has been well remarked by Captain Basil Hall, in the minute and luminous description which he has given us of these celebrated falls, that the river on which they occur is unlike any other stream with which we are at present acquainted. It is at its full size from the first moment of its existence; and although it contains more water than almost any other river, at the same distance from the sea, it is no larger at its mouth than at its commencement. Its course, too, instead of being of vast extent, proportional to the body of water of which it consists, is not more than about 36 miles. This anomaly is, however, at once explained when we consider that the Niagara is but a communicating link between two of the great American lakes; and that in its short and turbulent course, it has neither time nor space to acquire nourishment from subsidiary streams.

It is a common remark of strangers who are proceeding to view this wonderful cataract that it seems unaccountable how such a phænomenon can exist in a country which appears, on all sides, to present so level and unvaried a surface. In passing down the river from Lake Erie, the banks, thickly clothed with forests, seem on a perfect level with the surface of the water, and the general tameness of the scenery is not such as is well calculated to introduce the traveller to so speedy and violent a change. In approaching in a cross direction, the roads also pass through level and extensive woods, as little indicating a change of surface; and it is only on approaching from the side of Lake Ontario, and in passing up the stream, that the gentle inclination of the country which has given rise to the cataract is distinctly perceptible.

When we consider, indeed, the nature of this country, and view it on that great scale which alone is suitable to its vast expanse, it will be at once acknowledged that the whole globe may be passed under our review, without exhibiting any si-

* Communicated by the Author.

milar index to the length of time which has elapsed since the first commencement of the present state of things on the surface, and consequently having so direct and simple a bearing on the geological character of the district in which it is found.

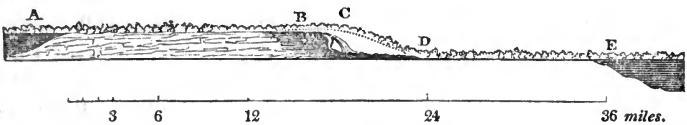
It is perhaps not so well known as it ought to be, that almost the whole continent of North America consists of vast plains, composed of secondary strata of various kinds; and that calcareous formations in a horizontal stratification form the leading characteristics in the geology of that country. The prodigious lakes of Canada and the United States are formed by the easy and gentle undulations of these secondary strata; and the sea-like extent of these freshwater basins, together with their generally low and swampy shores, exactly correspond with the boundless extent of plain which surrounds them on every side. The very circumstance of the rivers of North America being navigable for so great a proportion of their length, is sufficient to show the nature of the great steppes through which they take their easy course towards the ocean; and the *rapids*, which form so interesting and picturesque a feature in the inland navigation of the New World, are obviously occasioned by those breaks and interruptions in the superficial strata for which calcareous formations are so remarkable.

It is to one of these interruptions in the general level of that part of the world that the cataract of Niagara owes its origin. Two vast plains, or *steppes* (as they are termed in the North of Europe), extend themselves in different directions. One is spread over Upper Canada and New York towards the north, while the other embraces the shores of Lake Erie and its surrounding States towards the south-west. Between these great plains there is a considerable difference of level; and as the former is lower than the latter, all the waters which are drained from the one must experience a fall, more or less violent according to the nature of the line of demarcation over which they must pass, before they can subside into the general level of the other.

In this particular case it happens that this line of distinction between these two plains is situated longitudinally between the two great lakes of Erie and Ontario, which are not further removed from each other than about 36 miles; and as the difference of level is spread over an easy slope of 10 miles, and does not amount to more than 330 feet, (or 1 foot of rise in 160 feet of length,) the variation in the surface is so imperceptible, unless in the neighbourhood of the rivers, that it is not easily detected by a common observer in passing through the country.

This state of the case will be more clearly understood

by the following section, in which the difference of level in the two lakes is exhibited, although, of course, on a scale of height altogether disproportioned to the extent of country over which the declivity is spread. This fall of 1 foot in 160 would not be thought remarkable in any inland stream of common size; but in the case of such a vast body of water as the Niagara contains, (which forms a large proportion of the whole fresh water of the earth, and has been calculated at *one hundred millions of tons* per hour,) the action, when once set in motion, must be enormous; nor can we feel surprise at the powerful effects which it produces, and on which I am now about to remark.



A. Lake Erie.
B. The rapids above the cataract, having a fall of about........ 55 feet.
C. The falls of Niagara.. 160
From C to Queenston, D, is about 7 miles of violent rapid, gradually subsiding, and having a fall altogether of........ 115

Total 330 feet.

The very trifling fall in the navigable parts of these 36 miles is not here taken into account, but may be 2 or 3 feet of the above number. They extend from A to B, and from D to E, or Lake Ontario.

But first, in order more clearly to illustrate the nature of the country and the situation of the cataract, I shall quote a short passage from Captain Hall's interesting account of it, which is in itself perfectly clear, and has been corroborated to myself by the oral testimony of my friend Sir Howard Douglas, whose intimate acquaintance with the locality, and whose acute discrimination on all such subjects, no one will dispute.

" In the first part of the course of the Niagara, this river slips quietly along out of Lake Erie *nearly at the level of the surrounding flat country*; so nearly so, indeed, that if by any of those causes which swell other rivers, but have no effect here, the Niagara were to rise 8 or 10 feet perpendicularly, the adjacent portion of Upper Canada on the west, and of the State of New York on the east, would be completely laid under water. After the river passes over the falls, however, its character is immediately and completely changed. It then runs furiously along the bottom of a deep wall-sided valley, or huge trench, *which seems to have been cut into the horizontal strata of the limestone rock, by the continued action of the stream during the lapse of ages.* The cliffs on each side are,

at most places, nearly perpendicular, without any interval being left between them and the river, or any rounding of the edges at the top; and a *rent* would seem a more appropriate term than a valley*. Above the falls, therefore, that is, between them and Lake Erie, *there is literally no valley at all*, as the river flows with a gentle current, and almost flush, as seamen call it, or level with its banks; while below the cataract, the bed of the river lies so deep in the earth, that a stranger unprepared from these peculiarities, is not aware of there being any break in the ground at all, till he comes within a few yards of the very edge of the precipice."

Such is the clear and concise statement of Captain Hall; but it must be understood that this deep and precipitous trough does not extend much further than Queenston Ferry, 7 miles below the fall, and where the current flows at the rate of about 3 miles an hour. The sides, during the whole of this descending course, become gradually lower, until at length the river and its banks, on approaching Lake Ontario, reassume the same level and peaceful character which had been remarked for nearly 18 miles below Lake Erie, until disturbed by the rapids above the fall.

It is clear, therefore, that the waters of the whole of the upper lakes, in seeking their level in the ocean, have to descend from the higher to the lower plain; and as this descent does not take place by a wide valley, such as forms the usual channel for most other rivers, it is equally obvious that *a period once existed*, *when these waters first began to overflow*, and when they must have made their way *over the upper surface of the country in the form of a great rapid*, (see the dotted line from B to D of the section,) and that the violence of this superficial action on secondary strata of a horizontal form, has gradually occasioned a cataract, which would naturally commence near the base, and which has in the course of many ages gradually worked its position *backwards*, until we now find it nearly at the greatest possible height which the nature of the ground will admit of. If this point be admitted, it is equally obvious that a continuance of the action must occasion a continuance of the effect, and that a time must consequently arrive when the whole barrier between the two lakes will be intersected. This period is, of course, very remote†; but it is not the less certain and unavoidable, if the

* A *trough* would, perhaps, be a more suitable term than either, being the result of continued action. A rent implies a sudden effect of some violent convulsion, of which that entire district offers no one instance.

† The distance being 21 miles from the present cataract to Lake Erie, and the rate of action being about 4 feet per annum, the time necessary for this great natural operation will be 27,720 years. As the fall will,

causes now in force continue to exist. The consequences will be most extensive and disastrous, more so, indeed, than any *natural* event within the range of history. The whole of the upper lakes of North America, which more resemble seas than inland collections of fresh water, will then be lowered by nearly 300 feet, and the low countries between Lake Ontario and the Atlantic can scarcely escape from being swept at once into the ocean.

But it is not my present object to look at this subject prospectively. We shall find the retrospect equally distinct, and infinitely more instructive, and I shall therefore now resume that view of the matter.

No one who has examined and described the phænomena of Niagara has ever doubted that the section of the strata has been effected by the sole action of the continued current; and we have only to consider the nature of the existing causes and effects, as exhibited at the cataract and below it, to be convinced that this is unquestionably the case. Had this celebrated fall been situated in the Old World,—for example, in Italy, in India, or in Egypt,—we should have been enabled to calculate with precision the rapidity of its retrograde movement, by means of the ancient temples and other buildings which would probably have marked its former site in remote periods. Not having this advantage, however, and finding it situated in a region which, but a century ago, had never been visited except by the hunter or the roving Indian, we are compelled to adopt as a basis for our calculation the observed effects now in daily progress, and the evidence of respectable individuals who have resided for the last 40 or 50 years in the immediate neighbourhood of the spot.

When Captain Hall was informed, by authorities upon which he could confide, that the cataract had receded *about* 50 *yards in the last* 40 *years*, he found great difficulty in giving credit to so rapid an action, being little short of 4 *feet per annum*: and he was consequently led to a minute examination of the nature of the rock and the direction of its stratification, when he at once perceived and admitted that this rate of action had probably not been over stated. He describes the rock as calcareous, and the stratification as quite horizontal, as is usual with such secondary formations. Near the top, the stone is hard, but as the examination is carried downwards, the solid stone ceases, and a crumbling shale succeeds at the depth of

however, be higher than it now is when it reaches the top of the rapids, the action cannot be calculated at so much as it now is, and the United States on the coast may therefore safely reckon on a lease of from 30,000 to 40,000 years.

about 100 feet; and this softer substance, being acted upon by constant moisture and a violent current both of air and of water, is rapidly decomposed, and gradually leaves the harder beds overhanging their base to the amount of 50 or 60 feet. This excavating effect produces that species of gallery into which the more intrepid visitors can enter for about 150 feet, and in front of which the appalling cataract tumbles like an agitated curtain. It is clear that this excavating effect cannot be carried on beyond a certain point; for when the upper strata are, at length, no longer able to sustain their mighty load of waters, the whole table rock must give way, and a similar process be again renewed. By such incessant action, the form of the cataract is constantly changing. Many instances have occurred of late years; and in 1828 a fragment of rock gave way, and was carried into the abyss, which was calculated at not less than 5000 cubic yards, producing repeated shocks as of an earthquake, which were felt to a considerable distance in the surrounding country*.

The receding of this remarkable waterfall being thus established, both by the evidence of facts and by the very nature of the case itself, the question at once arises as to *the length of time required for executing the seven miles which have already been completed*; and it is a self-evident fact, that however long or short this period may be found to be, *it must have commenced at zero*; or, in other words, the commencement of this great natural work must distinctly point out the commencement of the present system of things *on the whole continent of North America*; and, by reasonable analogy, in other

* "The epithet of the *horse-shoe*," says a recent traveller, "is no longer applicable to the greater fall. In the progress of those changes *which are continually taking place from the attrition of the cataract*, it has assumed a form which I should describe as that of a semihexagon."—*Men and Manners in America*, vol. ii. p. 320.

"Nothing which enters the awful cauldron of the fall is ever seen to emerge from it. Of three gun-boats which were sent over the falls some years after the termination of the war, one fragment only, about a foot in length, ever was discovered. It was found at Kingston, about a month after the descent of the vessels."—*Ibid*. p. 328.

The author of "Transatlantic Sketches" says, "The American fall seems fast assuming the horse-shoe form. In standing under the falls, one constantly hears the sound of falling rocks amidst the awful roar of the cataract; but many of these may have been rolled down the rapids from a distance, and may not be portions of the rock of the cascade itself." Vol. ii. p. 155.—This, indeed, is highly probable; but at the same time, every falling stone at this cataract, even from the rapids above, tends to forward the great natural section which we are now considering. *None come from Lake Erie*, nor from the smooth course of the river for 12 or 14 miles below that lake; and there must obviously always be a rapid in preparation above the fall, and retrograding along with it.

parts of the world also, where the superficies is found to possess a similar secondary character. This being the necessary result from our inquiry, no one will probably deny that its bearing upon the science of geology is most important; and that, in fact, it involves a train of reasoning which must powerfully influence the theories of that most interesting branch of study.

It has been already stated that the distance from the fall to Queenston Ferry, where the river has resumed its more tranquil and navigable course, is about seven miles, or 37,000 feet; and if we divide this distance according to the data already stated, or at the rate of 4 feet *per annum*, we find that a little more than 9000 years would be necessary to complete the section as we now find it. It is scarcely necessary, however, to point out that such calculation would not be fairly stating the facts of the case. As we now see the fall, we find that it has attained within 50 perpendicular feet of the summit level, and that it is now, consequently, acting upon a vastly greater resisting body than was at first opposed to it, in the commencement of its labours. By a reference to the section, it will be seen that the height of the fall must necessarily have progressively increased, from the very bottom at D, where it begun as the lower portion of a *mere superficial rapid*, up to its present elevation of 160 feet; and as the acting force has, at all times, been *the same*, while, on the contrary, the opposing one has been *greatly less*, it would by no means demonstrate the truth if we took the present rate of action as a fixed point to calculate from. We arrive at this conclusion by a natural and obvious line of reasoning, which we should not hesitate to apply in the case of any great artificial work. In this difficulty it naturally occurs to the mind to try the question by the most approved system of chronology, and it therefore only remains to divide these 37,000 feet by the 4000 years, more or less, which are generally supposed, even by many geologists (and Cuvier amongst the number), to have elapsed since the period of the LAST great catastrophe (geologically speaking), or the Mosaic Deluge (if we trust to the evidence of Scripture). We find, then, that by this process *a rate of action, amounting to about* 9 *feet per annum, would exactly bring us to this great and interesting point*; and it only remains for those who are accustomed to such calculations of solid measurements to say, whether 9 feet of *general average* over the whole space may be admitted to be a fair medium amount of action. If, by *manual* or other labour, 4 feet are removed *per annum* at A, a *cube* of the same dimensions would require a little more than 9000

years to remove the solid contents at the same rate: but as in the case now before us, *only about half the cube is to be removed*, we may fairly conclude that the work would be completed *in one half the time, or in about* 4500 YEARS*.

Having attained this very interesting and remarkable result, which I cannot help thinking is beyond the reach of cavil, or of serious dispute, the mind naturally refers to the probable state of things *before the commencement* of this great natural operation; that is, and *necessarily must be*, before the existence of the North American lakes, and before the calcareous beds which now cover the plains of that vast continent *were elevated above the sea* in which it is admitted they must have been deposited. We find in the phænomena of Niagara one of the most striking corroborations of that evidence which history and the traditions of all lands, civilized and savage, have handed down to us, and which the fossil remains of animals of the *same species*, and buried under circumstances precisely similar on the level of the present sea, and at 10,000 feet above its surface, so distinctly illustrate. The theories of many able geologists have been distinctly opposed to the fact of the Mosaic Deluge; and one very talented author, for whose abilities I have a high respect, has in a late publication candidly announced, that he has "no hesitation in saying, that it has never been proved to have produced a single existing appearance of any kind, and that it ought to be struck out of the list of geological causes." I make no remark upon this passage; and I only quote it for the purpose of showing the opinions of high authorities that are abroad upon this subject. It will give me the greatest pleasure to be set right in the arguments which I have ventured to draw from various distinct, *and otherwise unaccountable*, sources in

* A cause and effect in some degree similar may evidently be traced on a large proportion of our secondary coasts, where, though a precipitous range of cliffs is now commonly found, a regular continuance of the existing slopes must at some former period have extended to the surface of the water, and formed the margin of the ocean's bed. We see this clearly demonstrated wherever such secondary heights have been protected from the continued action of the waves, and the aiding corrosion of the atmosphere, by the forms of bays, and the lowness of the sea-beach, common in such situations. In such circumstances we often find a sloping elevation in its *perfect* form on one hand where the sea has never acted upon it, and with a precipitous cliff on the other, although a long extent of gradual inland slope assures us that this sudden and precipitous cliff has not always existed. We can perceive the annual progress of this corroding action, and if we, therefore, carry on the outline pointed out by the regularity of the inland slope, until it touch the surface of the ocean, we shall have a case of gradually receding destruction, extremely similar in effect, though different in cause, to the remarkable cataract we are now considering.

support of the Scripture statement; and *last*, though not *least*, from the above phænomena of the greatest of known cataracts; and I shall look with some anxiety for a simple and consistent refutation of the subject of this paper. In the mean time I must confess my own complete conviction of the importance of these facts to the science of geology. We everywhere hear of the *immense periods*, amounting to millions of years, in which geologists seem to delight to revel. We hear of a time when the mighty Mastodon and the gigantic Mammoth, as well as other animals now supposed to be extinct, *lived* and *died* upon the very spots in which we now find their remains in a *shattered* and *detached* state; we find, upon the same authorities, that the great plains of the American continent must have been thickly peopled by these interesting animals; and we derive a sort of mysterious pleasure in diving into the recesses of past times, and in picturing to the imagination the state of the present lands of the earth at periods supposed to be far anterior to the creation of the human, or of most of the existing animal, races. In the midst of these pleasing day-dreams we are often disturbed by the contradictory and stubborn facts which are from time to time brought to light in opposition to them; and to crown the whole, we find our magnificent ideas of the former state of the American continent threatened with sudden and complete annihilation, by the evidence of Niagara *against the very existence of the plains of that continent above the surface of the ocean before the Mosaic Deluge.*

To such, however, as have viewed this important subject in the light shed upon it, with a steady and distinct illumination, by Scripture, this remarkable result is but a new and powerful link in the mighty chain of facts which, however shaken, can never be broken. Our minds are preoccupied with a certain set of theoretical data, upon which the most glaring, though opposing, facts appear, too often, to have little or no influence. Such facts must, however, exert, sooner or later, an increasing force, and must in time lead to a more consistent and natural (not to say Scriptural) view of things. Here, for instance, is a fact which seems to bear its own date and history on its front with almost as much distinctness as if they had been written in letters of brass, and intended for an index to the period when the present surface of this and many other secondary countries first issued from their parent deep. We have only to cast a glance over the nature of the great American plains to be convinced that this is the case. These vast steppes extend from the western slope of the Alleghany mountains to the sand plains, a distance of 1500

miles, and from the northern lakes to the mouth of the Ohio, a width of nearly 600 miles. They have been so well described in the second Number of the Illinois Monthly Magazine, as quoted by Mr. Stuart in his late able work, that I cannot resist inserting an outline of the statement. These plains embrace the States of Ohio, Indiana, Illinois, Missouri, Kentucky, and parts of Pennsylvania, Virginia, Tennessee, Arkansas, and Michigan, as well as a wild region of about 500 miles wide, lying to the west of these States. No part of the globe, perhaps, presents so uniform an extent of fertility. There are no sterile districts, *no rocky or precipitous ridges*, and but few swamps to deform so fair a surface. This uninterrupted fertility is stated to arise from the decomposition of the great limestone pan on which they repose, and which pan, we have seen, is in part intersected by the action of the waters of Niagara. This whole level region, though traversed by numerous rivers, is not a valley, or a system of valleys, but a real steppe, or plain, over the whole of which there is but a very slight difference of elevation. The N.E. corner of the plain near Pittsburgh, in Pennsylvania, is about 800 feet above the level of the sea; the plain of Kentucky and Tennessee is about the same height; that of the Ohio is but little different; and to the westward of the Missouri and Arkansas to the sand plains, the same conclusions force themselves upon us.

The great and numerous rivers which cross these plains, instead of forming distinct valleys, do but indent narrow scratches or grooves into the surface, barely sufficient to contain their waters. These river channels *have formed a declivity for themselves*, and towards their terminations they sink deeper into the plain: hence the larger rivers *appear* to be bordered by abrupt hills of several hundred feet of elevation; but this is, in fact, only in appearance; the tops of these heights are but the general level of the great plains.

The following short outline of the general geology of these countries is given in the Magazine above mentioned. "The formation of these plains is decidedly secondary, reposing on a horizontal limestone rock, the thick strata of which *have never yet been penetrated*, although the auger has pierced in many different places from 400 to 600 feet in search of *salt water*. This limestone is hard and stratified, imbedding innumerable *sea shells* of the *Terebratulæ*, *Encrinites*, *Orthoceratites*, *Trilobites*, *Productæ*, &c. This limestone pan is generally but a few feet below the surface, and SUPPORTS *strata of bituminous coal* and saline impregnations through almost its whole extent. The decomposition of its parts has fertilized

this wide region*, and its absorbent and *cavernous* nature prevents swamps and moisture from accumulating on its surface. The mineral resources of these plains are unbounded, *and its coal-field would cover half Europe.* The coal is pure, *lies above the level of the river channels*, and is easily worked. Iron and lead are also abundant. *Salt water* is found over the whole extent, and yields from $\frac{1}{8}$th to $\frac{1}{12}$th of its weight in pure muriate of soda. This salt water breaks out in many places in the form of springs; but it is more usually necessary to bore to the depth of from 3 to 600 feet into the limestone. Gypsum and saltpetre are found in abundance, and most of the earths or clays useful in the arts."

There is probably no part of the world of a similar extent that more completely points out its own history and formation than these North American plains. We here find reposing on a *marine* formation, the depth of which is unknown, and which remains in an undisturbed level surface, all the marks we can desire or expect of *diluvial* deposits in the form of deep beds of clay and gravel containing the fossil remains of the Elephant, the Mastodon, and other contemporaneous animals; and also extensive strata of pure vegetable and bituminous coal, as well as the chemical formations of ironstone, gypsum, lead, &c. From the great extent and level nature of the original sea bed here laid dry, the diluvial deposits have also assumed the same regularity of form; so that the researches of the geologist are infinitely less complex than in our own island, where we have, crowded into a small district, a confused mixture of every description, which has given rise to much difficulty, and to a wide field of theoretical speculation, contributing largely to give to this interesting study that doubtful and obscure character of which it is, in fact, undeserving, when viewed upon a wide and proper basis. Had this science taken its rise in America instead of in Italy and in England, the more enlarged scale of things, and the more simple nature of the formations, would have led to that corresponding simplicity which true geology possesses. But in the *microscopic* views of things which are alone within the reach of our personal inspection, in the most scientific parts of Europe, we can scarcely wonder at the extraordinary ideas which have arisen on this subject, and which have naturally spread with civilization into other more extensive regions, to which they cannot possibly be consistently applied. Instead, then, of making our confined districts the arena of geological illustra-

* This is a point which is more than questionable. The fertility of this wide region is in great part owing to the alluvial or *diluvial* soils which cover its whole extent to a greater or less depth, and in which the fossil remains of Mammalia unnatural to America are so constantly found.

tion, and then extending the analogy to the boundless continents of the earth, would it not be wiser to study this interesting subject on the larger scale, and thus become enabled to apply it with greater precision to the more confined districts in which we happen to be placed? Let us hope that the theories of geology are not yet so firmly established in the public mind as to place this more consistent method entirely beyond our reach. Let us, by such obvious steps as I have now been endeavouring to establish, go *backwards* in our researches. Let us first firmly establish the fact of a universal deluge about the very period denoted by Scripture chronology. When this great point is admitted, and proved beyond cavil (which is not supposing too much), let us candidly apply to such diluvial waters, the full power and action of the *tides*, and more especially of the *currents*, by which our seas are kept in such continual circulation, and which are a natural and necessary consequence of the rotatory motion of a *solid* globe covered by a *fluid* ocean. Let this action be supposed to have continued for the full period mentioned in the record; and on the restoration of the whole system, from its preternatural to its natural course, let us view the probable and even *unavoidable* state of the new dry lands, not upon the scale of our trifling rivers and floods in England, but upon that vast and extended scale exhibited on the continents, which is, however, only clearly pointed out to the mind's eye, by the idea of the whole earth being surrounded by a boundless and agitated ocean. We shall then think less than we now do of some *hundreds of feet* of stratified aqueous deposits; we shall then perceive a consistent reason for our vast coal-fields, with their *vertical* witnesses of *rapid formation*, to which I have adverted in a former paper. Where the ocean's bed had been rough and disordered, there we must expect to find a proportional complication of sedimentary matter. Where, on the other hand, it was more level and equal, (as in the case of the American plains above described,) there we shall look for such results, and such a simple system of stratification, as are now presented to our contemplation.

We find a distinct index, as to *time*, in the section worked out by the cataract of Niagara. We find this section to have been begun, as it naturally must have been, immediately subsequent to the restoration of order, after the Mosaic Deluge. We find the river cutting its own course through a *marine* formation of an unknown depth, but extending beyond *two hundred fathoms*, and which spreads continuously over plains on which all Europe could be placed; and, lastly, we find *on the surface* of this marine formation, beds of bituminous coal of the purest kind, and of wonderful extent, together with

the fossil remains of many animals unnatural to America, but all of which must have been deposited there *at the period when the waters of Niagara were first set in motion*, and consequently they must all have been laid where we now find their *shattered* remains by the action of the universal deluge.

It is unnecessary in this place to follow out at greater length this simple and obvious line of reasoning, or to extend it by analogy to other parts of the present dry lands of the earth. No reader who is satisfied of its truth will have any difficulty in applying it to every other district, and especially to England, where our coal-fields, though less simple, are yet full of proofs of diluvial action; and where our fossil remains of land animals can always be simply accounted for upon this principle, and upon no other that is not beset with difficulties, and contradictions of the most glaring kind. I shall therefore conclude this paper with the testimony of the illustrious Cuvier to the truth of the point for which I am now contending, although he drew from it very different conclusions. "I conclude," says he, "with M. de Luc, and with M. Dolomieu, that if there be any fact well established in geology, it is this, that the surface of our globe has suffered a great and sudden revolution, the period of which cannot be dated further back than 5 or 6000 years. *This revolution has, on the one hand, ingulfed and caused to disappear the countries formerly inhabited by men, and the animal species at present best known; and on the other, has laid bare the bottom of the last ocean, thus converting its channel into the now habitable earth.*"—*Disc. Préliminaire.**

Ramsgate, July 23, 1833.

Appendix.

Since the above paper was written, several months ago, a doubt has been suggested to me, by some able observers of the falls of Niagara, whether the section of the seven miles down to Queenston from the falls can be properly regarded as only *the half* of a solid oblong square, as above stated. The general slope of the country is supposed to incline downwards from the falls, but not in so regular a manner as I had been led to believe; and it comes to a more abrupt and decided termination near Queenston, at what are termed Queenston heights, where the slope is more rapid, and where it falls into the general level of the great plain of Lake Ontario.

[* Some remarks by the Rev. W. D. Conybeare on Mr. Lyell's estimate of the time occupied by the retrogression of the falls of Niagara from their original to their actnal position, will be found in the Phil. Mag. and Annals, N.S., vol. ix. p. 267; the subject is also noticed in vol. x. p..316. Mr. Lyell, we observe, calculates that the retrogression occupied 10,000 years.—Edit.]

This rapid slope, however, is by no means *precipitous*: it extends across the river on both sides, and is covered with forests and vegetation. It is from this point that the section made by the river takes its rise; and instead of so long a rapid as I had at one time been led to imagine, I have now reason to believe that *the primitive rapid* was chiefly over the superficial slope of Queenston heights, and that the form of the section would therefore be more correctly drawn thus,

than as given in p. 17.

It may be urged that this addition to the work to be performed must make a vast difference in the time required for the execution of it. This is not the result, however, in this case, although in common circumstances it might be so expected. But it has been observed by the same persons to whom I am so much indebted for this correction, that in the case of Niagara, my computation, as to time, has every appearance of being exceedingly correct. Not only is the deep ravine already cut out *much narrower* below the falls than at the cataract itself, but the crumbling shale which forms the lower part of the rock at the falls, forms, lower down, a much greater proportion of the whole rock. At the falls, the depth of the solid limestone is 70 feet, the whole of the remainder being of the soft nature of shale; towards Queenston, the superincumbent limestone is much thinner, and the action of the water upon the whole body of the rock must have been consequently much more rapid.

Again, as to the actual cubic measurement influenced by the river's action lower down, as compared with what it now is at the falls, there is a difference so great, that it would compensate for even a greater additional cubic body of rock than I have now found it necessary to add to my former calculation. The width of space over which the river's action is now spread, including Goat's Island, is about 3500 *feet*, and over this wide space, we find that the fall is retrograding at the rate of nearly *four feet* per annum. But the medium width from the falls to Queenston is not more than about 1200 *feet*, in some places more, in some less; and when this greatly inferior width is added to the softer nature of the rock already alluded to, we cannot doubt that the action was much more rapid in the early periods of the section, since the quantity of water must always have been what it now is.

We might, therefore, even in the absence of all other evidence, safely assume that the time necessary for the completion of this vast natural work could not be extended much above 4000 years. When this remarkable fact is corroborated,

however, by an exactly similar result in regarding the phænomena of our own and other sea cliffs, acted on by the sea, which will form the subject of another paper, all doubt on this head that may have existed must surely be for ever removed.

III. *On the alleged Greek Traditions of the Deluge. By the Rev.* John Kenrick, *M.A.*

[Continued from vol. iv. p. 420; and concluded.]

WE have now reached the time when the Greeks, having been brought into much more extensive and leisurely intercourse with Asia than they had enjoyed before the time of Alexander, had the opportunity of comparing their own fables with those of the Asiatics, and of remodelling or adding to their own, in order to give them a more imposing appearance, or to maintain the claim of originality in everything, to which in earlier times they were far from pretending, and which is, indeed, without any foundation in history. We cannot, therefore, appeal with confidence to what we find in Greek authors from this time forward, even as proving the existence of a Greek tradition; still less can Ovid's version of the story be appealed to. "A mesure," says Cuvier, "que l'on avance vers des auteurs plus récens, il s'y ajoute des circonstances de détail qui ressemblent davantage à celles que rapporte Moïse. Ainsi Apollodore donne à Deucalion un coffre pour moyen de salut; Plutarque* parle des colombes par lesquelles il cherchait à savoir si les eaux s'étaient retirées, et Lucien† des animaux de toute espèce qu'il avait embarqués avec lui," etc. Yet he had written in a preceding page, "Pour peu que l'on suive la manière dont le déluge de Deucalion a été introduit dans les poëmes des Grecs et les divers détails dont il s'est trouvé successivement enrichi, il devient sensible que ce n'était qu'une tradition du grand cataclisme, altérée et placée par les Hellènes á l'époque où ils plaçaient aussi Deucalion." If we find ingredients in the stream at a distance from the source, which it has not when examined nearer to the spring, the natural conclusion would seem to be that it has acquired them on its way, not that they were there from the first, in some mysterious state of delitescence.

* Plut. Πότερα τῶν ζώων Φρονιμ. § 13. viii. 930, ed. Wyttenb.

† It is doubtful whether Lucian were the author of the treatise *De Deâ Syrâ*, in which this mention of the flood occurs: what is more important to our purpose is that the writer, whoever he was, probably was an Asiatic Greek.

The traditions of the flood of Ogyges will not detain us long. Even his name, as a king of Attica, does not occur in any extant author before the time of Alexander, and for most of what we know of him we are indebted to the Christian chronologers. A passage in Eusebius, *Præp. Evang.* x. p. 10. might lead us to conclude that the flood in his time had been mentioned by Acusilaus, *i. e.* in the beginning of the 5th century B. C.: Ἀπὸ Ὠγύγου τοῦ παρ' ἐκείνοις (Ἀττικοῖς) αὐτόχθονος πιστευθέντος, ἐφ' οὗ γέγονεν ὁ μέγας καὶ πρῶτος ἐν τῇ Ἀττικῇ κατακλυσμὸς Φορωνέως Ἀργείων βασιλεύοντος, ὡς Ἀκουσίλαος ἱστορεῖ, μέχρι πρώτης Ὀλυμπιάδος, κ. τ. λ. But it is difficult to know what were the exact words of the original, and it may be that the mention of the deluge proceeds from Eusebius himself, especially as Syncellus has the following passage, (p. 119. ed. Bonn.): ὥστε οὐδὲν ἀξιομνημόνευτον Ἕλλησιν ἱστορεῖται πρὸ Ὠγύγου, πλὴν Φορωνέως τοῦ συγχρονίσαντος αὐτῷ καὶ Ἰνάχου τοῦ Φορωνέως πατρὸς, ὅς πρῶτος Ἄργους ἐβασίλευσεν, ὡς Ἀκουσίλαος ἱστορεῖ. I am therefore inclined to think that Acusilaus is properly to be considered only as an evidence to the dominion of Phoroneus at Argos; or at most to the synchronism between him and Ogyges. The name of Ogyges never occurs in Attic poetry, nor is there any trace of him in the remains of Attic art. Philochorus, who wrote his Atthis about 260 B.C. seems to speak of Ogyges as king of Attica, according to the quotation from Julius Africanus in Syncellus, (p. 281. ed. Bonn.): Τὸν γὰρ Ὠγύγου Ἀκταῖον, ἢ τὰ πλασσόμενα τῶν ὀνομάτων οὐδὲ γενέσθαι φησὶν ὁ Φιλόχορος. Yet even here I feel by no means certain that τὸν Ὠγύγου are not the words of the chronologer*. There is no mention of Ogyges in the Parian marble, which was engraved about the time when Philochorus published his Atthis.

* If any one should think that this is an attempt to get rid unfairly of a witness, let him see what a license this same chronologer allows himself. Μέμνηται δὲ καὶ Ἡρόδοτος τῆς ἀποστασίας ταύτης (the defection of the Jews under Moses) καὶ Ἀμώσιος ἐν τῇ δευτέρᾳ, τρόπῳ δέ τινι καὶ Ἰουδαίων, ἐν τοῖς περιτεμνομένοις αὐτοὺς καταριθμῶν. *Jul. Afr. ap. Sync.* p. 281. If we had no other knowledge of the Second Book of Herodotus than from this passage, could we have ventured to doubt whether the name of Amosis, if not of the Jews, occurred in it? Heyne, *Apoll.* vol. ii. p. 320, on the strength of this passage in *Jul. Afr.*, supposes that something has dropt out of the text of Apollodorus, at the beginning of 3. 14. in which, if we had it, we should find something about Ogyges. Yet the actual commencement of that chapter, Κέκροψ αὐτόχθων τῆς Ἀττικῆς ἐβασίλευσε πρῶτος, καὶ τὴν γῆν, πρότερον λεγομένην Ἀκτὴν ἀφ' ἑαυτοῦ Κεκροπίαν ὠνόμασεν, does not look as if it had ever been preceded by an account of Ogyges, a predecessor of Cecrops. With the same inaccuracy Syncellus (i. 238.) makes Plato in the *Timæus* speak of "the flood in the time of Ogyges," though Plato never mentions his name.

I have given in the Philological Museum, ii. p. 348, my reasons for thinking that the name and story of Ogyges belong properly to Bœotia. Whether I am right or not in the interpretation which I have given to his name, the fact that traditions respecting him were connected with Bœotia, and that we have no proof that they were so with Attica, remains the same. As to his flood, till some more decisive passages are produced than those which have been given above, I must be allowed to doubt whether we have any proof that either in Bœotia or Attica there was any tradition respecting it. Pausanias, says, *Bœot.* 5., that Thebes derived the name of Ogygian, which many of the poets had given it, from an autochthon, Ogygus, king of the Hectenes, and that this people perished λοιμώδει νόσῳ.

I have now to inquire how the popular belief among the Greeks upon the subject of a flood in the time of Deucalion is to be accounted for. Three suppositions may be made, The progenitors of the Hellenes brought with them from Asia a tradition of the flood of Noah, which they localized in their own country, by attributing it to Thessaly. This is the common opinion; and it is the more easily adopted because, as we know nothing whatever of the tribes from whom the Greeks originated, we pass *per saltum* from Thessaly to the plain of Shinar, and nothing seems simpler than that they should preserve a tradition of a recent and impressive event. But let us consider chronology a little. According to the Hebrew text the deluge is placed about 2300 B.C.; but historical inquirers are beginning to feel the inconvenient limitation of time which this occasions, and adopt the Septuagint reckoning, which will carry it up to 3500 B.C. Now we have not found any traces of this tradition in Greece earlier than the commencement of the 5th century before Christ. Here is then a period of 3000 years, during which we must suppose it to have been preserved in the minds of a race who, if their own tradition about Prometheus is to be believed, (and it is found in earlier authors than that of Deucalion,) had not even the use of fire among them. It would be too much to say that such a transmission is impossible. Those who believe that Ammonian and Cuthean priests marched from Chaldæa to all parts of the world, bearing diluvial traditions and helio-arkite symbols, may not even find it improbable; but let the linguist and the ethnographer reflect on the changes and vicissitudes through which the ancestors of the Hellenes must have passed, and they will be startled, I think, at the demand made upon their belief. Nothing could make the preservation of a tradition in these circumstances credible, but such a close resemblance as pre-

cluded the idea of accidental coincidence, manifesting itself at a time when there could be no communication with a foreign source. We have not found either of these conditions fulfilled in our inquiry into the traditions of Deucalion.

The next hypothesis which suggests itself is that they are the real reminiscences of some local flood, such as a country abounding with lakes and mountains, bordered by the sea, and exposed to earthquakes, must often have undergone. It is certain that the Greeks had no tradition of a general deluge. Apollodorus, whose description of the flood of Deucalion bears some resemblance to Noah's flood, says, *τὰ ἐκτὸς Ἰσθμοῦ καὶ Πελοποννήσου συνεχύθη πάντα*, on which Heyne observes, "in Peloponneso diluvii memoria nulla," which is substantially true, although Dionysius of Halicarnassus (i. 61.) mentions that some of the Pelasgians left Arcadia for Samothrace in consequence of a flood which deprived them of part of their land. This is also true of Attica, though Pausanias speaks of a chasm (i. 18. 43.) in the temple of Jupiter, through which the water of Deucalion's flood (*ἐπομβρία*) had run off. This may seem a presumption in favour of a real tradition of an inundation produced by an earthquake, or some similar cause. Yet even in this modified form, the opinion of a real tradition appears to be open to many of the objections urged against the former hypothesis. If chronology is to be applied to these matters at all, Deucalion's flood cannot be placed later than the 15th century before Christ: we find the first mention of it in the 5th. We must not judge of the probability of such a transmission by the fact of traditions having reached our own times from those of the Roman empire: the use of writing has never been lost in Europe. Besides, all the circumstances of the story are evidently fictitious. No inundation could have floated or driven Deucalion to the top of Parnassus without deluging all the low lands of Greece; and it will then require to be accounted for, how this event has left no tradition in the other parts which must have suffered from it. If we take away from the story Deucalion and Pyrrha, the ark, the resting on Mount Parnassus, the reproduction of the human race by the casting of stones, what remains to be the matter of a tradition? The simple fact of an inundation,—a natural phænomenon which the imagination can multiply and magnify as much as it pleases, and place in any age it thinks fit. The only reason for admitting such an extraordinary transmission of a fact, through ages which have preserved no memorials of their own history, would be the impossibility of conceiving how such a thing should have been invented, if not true. But unless

the event, though within the laws of nature, were beyond men's knowledge and experience of them, the more likely it was to happen, the more easy it was to suppose it happening, to adorn it with circumstances, and to fix it to time and place. The vulgar, too, draw much more liberally on extraordinary causes to explain appearances, than a philosopher allows himself to do.

If we are to receive the account of Deucalion's flood as the tradition of a real occurrence, it will be difficult to say why we should not do the same with regard to other Greek fables. But of what real occurrence is the combustion of the world by Phaethon, a tradition; or the submersion of the island Atlantis; or the splitting of the continent Lyctonia into the islands of the Mediterranean*? Although Sicily was to the Greeks "the Threepointed Island†," from the earliest time in which it is named by them, and Scylla and Charybdis appear in the voyage of Ulysses, must we suppose that there was a real tradition of its having been a part of Italy, separated from it at Rhegium‡ by an earthquake?

We seem, therefore, to be brought to the third supposition, that there is nothing historical in the flood of Deucalion, and that all the circumstances which we find in Greek authors respecting it, previously to the time when they may have mixed their own accounts with those of foreigners, are fictitious. But fictions must have a determining cause, and those which relate to physical events generally have this cause in physical appearances, popularly interpreted. Thessaly, the scene of Deucalion's flood, and Bœotia, to which some, though fainter, traditions of a similar event may have been attached, were both countries which, from their structure, were peculiarly liable to inundations. We are so accustomed to associate ideas derived from Scripture with the words *flood*, *deluge*, *cataclysm*, that we transfer them to other ancient history, and suppose that they imply events of equal magnitude. But any overflowing of a river which swept away what was upon its banks, was to the Greeks a *κατακλυσμός*. The floods of Morayshire, and many of much inferior extent, would have been called so by them. The physical structure of Thessaly rendered it, of all parts of Greece, the most natural scene of an inundation which should dislodge mankind from their customary abodes. The whole drainage of the valley which is inclosed by Pelion, Ossa, and Olympus, Pindus and Othrys, takes place through the single

* Orph. Argon. 1283 *seq.*

† At least if the Θρινακίη of Homer (Od. λ', 106. μ', 127.) be the Trinacria of later geography, which can hardly be doubted. See Uckert, I. i. p. 21.

‡ Strabo, i. p. 88. ed. Oxon.

outlet of the Vale of Tempe*; and it was not necessary to have the eye or the imagination of a geologist, in looking at this singular chasm, to perceive its use, and revert to the time when no such opening existed, and the waters of the Peneius, the Enipeus and the Apidanus must have discharged themselves into a lake covering the greater part of Thessaly. The well-known passage in Herodotus (vii. 129.) shows how early such an idea had been formed. It was then a λόγος that all Thessaly had been once a lake, and that Neptune, *i. e.* an earthquake, had opened the passage by which the lake was drained. That there was any real tradition of a time prior to the opening of Tempe, I do not believe, for the very first glimmerings of Grecian history show us Thessaly peopled by various tribes, whose seats were on the banks of the rivers; but if men speculated upon the condition of things which must have preceded the opening of the outlet, and put their speculations into the form of a λόγος, why might they not also speculate on the consequence of a sudden stoppage of the outlet, or of a fall of rain so copious and long continued that the narrow passage of the Peneius could not afford it vent? Such a speculation, turned into a λόγος, the form which ancient hypothesis generally assumed, would include all that belongs to the flood of Deucalion, separated from foreign admixtures. I have already mentioned that, as related by Apollodorus, the story appears to contain portions of two distinct *mythi*. Deucalion was probably at first only the patriarch of the Hellenic tribe; but as they had their original seat in Thessaly, and affected to consider the commencement of their own history as the commencement of civilization, the flood of Thessaly and the person of Deucalion would naturally be connected together†.

Bœotia, like Thessaly, is exposed, from its physical structure, to suffer from inundations. Its principal river, the Cephisus, terminates its course in the lake Copais, the waters of which have no superficial outlet to the sea, and would soon lay the whole country under water, were it not that they find

* See Hawkins in Walpole's Memoirs of Greece, vol. i. p. 528.

† Mr. Keightley in his Mythology, p. 268, derives Deucalion from δεύω (whence δεύκης) *to wet*, and perhaps ἅλς *the sea*. But besides the difficulty that δεύκης does not appear to mean *wet*, why should Deucalion, who rode dry in his λάρναξ and whose flood was one of rain water, be called by a name which means dipped in the sea? What if Δευκαλίων and the cognate form Δεύκαλος (*Heyne ad Il.* ν′. 307.) were derived from Δεύς, the old form of Ζεύς, and καλέω? The commencement of religion among the Hellenes would naturally be ascribed to the patriarch of the race, and he was not only reputed to have founded Dodona in Thesprotia and to have sacrificed to Jupiter on his deliverance, but even to have founded altars to the twelve great gods.—*Apollod.* 1. 7. 2. *Eudocia*, p. 108. 127.

a vent by a subterraneous passage in the cavernous limestone, and discharge themselves into the Euripus at Larymna. This subterraneous channel was liable to be stopped, especially by earthquakes, which were frequent in Bœotia, and the consequence was, an inundation of the country around the lake. In the time of Alexander the Great, an engineer, Crates of Chalcis, was employed to open the obstructed passage, and the shafts which he sunk down upon it may still be traced along its course*. In a report addressed to Alexander, preserved by Strabo, he says that many districts had already become dry, and the sites of ancient towns had reappeared. According to some, Eleusis and Athens had stood here in the time of Cecrops, who was king of Bœotia, then called Ogygia. Pausanias again (ix. 38.) relates that Hercules had stopped the subterraneous passage, in order to flood the fields of the Orchomenians. The singular condition of this lake, and the variations to which it was subject in winter and summer, to say nothing of extraordinary rains and accidental stoppages of the outlet, invited the fancy to frame wonderful tales respecting it, and we may surely account for the flood of Ogyges without having recourse to that of Noah. The wonderful coincidence in time, which made Cuvier (p. 85.) conclude that the former one must have been derived from the latter, really depends on a date arbitrarily fixed by the chronologers†.

I will mention one other opinion‡, which connects the stories of destruction by water and fire together, and supposes them to have originated in the observation, that at the different seasons of the year, more strongly discriminated in southern climates than in our own, the principles of moisture and heat alternately predominate, whence arose the notion that the *annus magnus*, which comprehended a vast cycle of celestial phænomena and terrestrial changes, included both a κατακλυσμός and an ἐκπύρωσις ||. This may have been the origin of the Egyptian doctrine and of that of Heraclitus and the Stoics; it may have had a share in producing the story of Phaethon; but the deluge and the conflagration do not appear to me to have had any connexion with each other in the popular conceptions of the Greeks, which alone it has been my object to examine.

The result of this examination will not, I hope, appear altogether unimportant. It is not difficult to foresee that the

* Walpole, i. p. 303. *seq.*

† Cuvier quotes Varro as placing the deluge of Ogyges 400 years before Inachus; but it does not appear from the passage in Censorinus (21.) that Varro mentioned the name of Ogyges.

‡ Bohlen, *Altes Indien*. i. p. 219.

|| Censorinus 18.

time is approaching, when geologists will no longer refer all marks of the superficial action of violent currents of water on the earth, subsequent to the consolidation of its newest strata, to one flood, limited in its time and strictly defined in its circumstances, as the Mosaic deluge is; but will be compelled to acknowledge that their science points to prolonged, repeated and multiform operations of diluvial currents*. The popular view of this subject derives a strong support from the belief that other nations have a real tradition of the Mosaic deluge, and that something like a chronological coincidence between them can be established. If this opinion has been shown to be unfounded, scientific inquiry into the phænomena of diluvial agency on the earth's surface will not be embarrassed by the necessity of making its results conform to those of another branch of knowledge.

I will only observe in conclusion, that the Mosaic account of the Deluge appears to me to bear many of those marks of a tradition of high antiquity, which we have sought in vain in the Greek legends of Deucalion. It is found in the book which the Jews have always regarded as the most ancient of their sacred writings, and it exhibits traces, as critics of the first name assure us, of being itself a document yet older than the book in which it has been incorporated. It does not correspond with the traditions or speculations of the Egyptians or the Phœnicians, but in a remarkable manner with those of the people of Mesopotamia and Chaldæa, countries with which the Jews were connected by the origin of their nation. Had they framed it for themselves, it would have been natural for them to refer it to their own country, to have made Noah build his ark in the forests of Lebanon, and the ark rest on the top of Hermon or of Carmel. Instead of this, everything is referred to the valley of the Euphrates and the Tigris. The cypress of which the ark was built was the only wood fit for ship-building which this region afforded†; the bitumen with which it was covered was the product of its asphaltic springs‡; the mountain on which it rested is that from the vicinity of which the Tigris and Euphrates rise, and which looks down

* [See Prof. Sedgwick's Anniversary Address to the Geological Society in 1831, Phil. Mag. and Annals, N.S. vol. ix. p. 313; and also Mr. Greenough's late Address to the same body, in our present Number.—Edit.]

† The testimony of Arrian, ix. 19., that the cypress was the only wood fit for ship-building of which there was any considerable quantity in Assyria, with the correspondence of the consonants in גפר and *κυπάρισσος*, is, I think, decisive in favour of the opinion of Bochart and Celsius that the *gopher* wood is the cypress. [See Mr. Beke's paper on this subject, in Lond. and Edinb. Phil. Mag. vol. iii. p. 103.—Edit.]

‡ Herod. i. 179. [See Mr. Beke's *Remarks* on Mr. Carter's paper, in our Number for April last, or vol. iv. p. 280.—Edit.]

over the countries through which they flow; the plain of Shinar is the scene in which the history of mankind recommences when the Deluge is over. Unless we could suppose the Pentateuch to have been written after the captivity, and the Jews to have begun their history with the borrowed traditions of their oppressors, we must admit that these things were subjects of belief in the family of Abraham from the time when he left his original abode in Ur of the Chaldees*.

IV. *Remarks on the Atomic Constitution of Elastic Fluids.* By WILLIAM CHARLES HENRY, *M.D., F.R.S.*†

THE following remarks, suggested by that portion of Dr. Prout's Bridgewater Treatise which is devoted to the most comprehensive generalizations of chemical philosophy, are proposed with considerable hesitation, from their not according with the views of that profound writer. But it must also be borne in mind, that the theory of atomic combination adopted by Dr. Prout differs itself, most materially, from that originally framed by the author of the atomic philosophy, and still held by him, as well as by the majority of British chemists. These differences, as far as they respect first principles, may be comprehended in the two following propositions:

1st, That equal volumes of all gaseous bodies contain, under the same temperature and pressure, the same number of self-repulsive molecules.

2nd, "That the self-repulsive molecule, as it exists in the gaseous form, does not represent the ultimate molecule, but is composed of many of them."

1st, The idea that the particles of all gaseous fluids are placed at the same distances from one another, and consequently that a given space contains in all the same number of molecules, seems to have occurred about the same time to MM. Ampère and Avogadro. It was published by the former, so early as the year 1814, in a letter addressed to Count Berthollet, but merely as the most probable hypothesis of the constitution of elastic matter‡. It was subsequently revived by Dumas, and has been recently maintained and illustrated by his pupil M. Gaudin§. Dr. Prout had arrived at the same

* [Our correspondent Mr. Beke, in the Appendix to his recently published work, entitled *Origines Biblicæ*, has shown reasons for the belief that the Flood, though universal with respect to mankind, was merely local with respect to the globe itself; a view of the subject which, if fully substantiated, would tend to relieve it from much of the difficulty in which it is at present involved.—EDIT.]

† Communicated by the Author. ‡ *Ann. de Chimie*, tom. xc. p. 47.

§ *Ann. de Chimie et de Phys.*, tom. lii. p. 113.

conception without being aware that it had been previously entertained by others. These distinguished chemists do not, however, concur with Ampère in regarding it in the light merely of an hypothesis, but conceive that it is strictly derivable from the well-known law of Mariotte, and from the similar relations of gaseous bodies to heat. They have therefore made the first of the above propositions the basis of their peculiar views on atomic combination, and have certainly succeeded in proving that the second and more important proposition flows from the first in direct logical sequence. It is therefore necessary to examine, with peculiar care, the grounds upon which the major term is supposed to rest.

The law of Mariotte, that in all elastic fluids the volumes vary inversely as the compressing forces, will be found to warrant no inference as to the number of atoms existing in a given volume of the different gases. It is derived from the law of variation observed by the repulsive forces which actuate the molecules of elastic fluids, not from the numerical aggregation of atoms in space. Newton has demonstrated (*Princ.*, Lib. II. Pr. xxiii.) "that particles flying from each other with forces that are reciprocally proportional to the distances of their centres, compose an elastic fluid whose *density is as the compression.*" Now this is the law of Mariotte, which is hence independent of all elements other than repulsive forces, varying inversely as the atomic distances or diameters. Whatever be the comparative distances of the particles of two gases A and B under any given pressure, the same for both, there must, in conformity with the law of Newton, be an equal diminution of their bulk on equal increments of pressure. For illustration, let us suppose the atoms of A to be at double the distance that exists between the atoms of B under the pressure of one atmosphere. Let the two gases be subjected to the pressure of an additional atmosphere. Then, since the molecular forces in both vary according to the *same* law, both gases will be alike reduced to half their original volume. But the number of atoms of B is eight times that of A. Hence it is manifest that the law of Mariotte has no reference whatever to the numerical relations of atoms in the different gases.

2nd, The argument founded on the equal expansion of the gases by heat, does not appear to be possessed of greater cogency. In the first place, it is consistent with the best recent experiments, as will be shown hereafter, that equal increments of *absolute heat* do not produce equal dilatations of volume in the different gases, and that this relation can only be correctly predicated of equal increments of *temperature*. Now there seems

more reason to anticipate the existence of such a relation between the number of molecules and the absolute or specific heat, than between that number and the heat of temperature. Upon this principle, since unequal increments of absolute heat are required to effect equal expansions, we should conclude that the numbers of atoms are also unequal.

That equal increments of *temperatures* should affect all elastic fluids in the same degree, is a manifest consequence of the constitution of such fluids. The unequal expansibility of bodies in the solid and liquid states is to be ascribed to the interference of the attractive forces which maintain those conditions of matter, and which counteract, with energies varying in different bodies and at different distances, the repulsive agency of heat. But the molecules of elastic fluids are separated to such a distance from one another that their mutual attractions become insensible *. They are therefore subjected to the undisturbed influence of repulsive forces. According to the theory of Laplace, caloric constitutes the sole agent of repulsion; and equal increments of temperature, being identical with equal increments of elasticity, are necessarily followed by equal expansions. "Sous une pression constante la densité d'un gaz étant, comme on l'a vu, réciproque à cette fonction de la température, son volume est proportionnel à cette fonction...la température est alors représentée par ce volume, et ses variations sont représentées par les variations du volume d'un gaz soumis à une pression constante."

It cannot be requisite to pursue this argument further, since it has been shown by Laplace, that both the law of Mariotte, and that of equal expansion discovered by Dalton and Gay-Lussac, are mathematically derivable from the following suppositions:—that the molecules of gases are at such a distance that their mutual attractions are insensible;—that these molecules retain caloric by a principle of attraction;—that their mutual repulsion is due to the repulsion of the molecules of caloric;—and, finally, that this repulsion is only sensible at imperceptible distances. If these suppositions be conceded, the laws of Mariotte and of Dalton are susceptible of rigid demonstration, and are moreover applicable to all elastic fluids, whatever be the nature or the number of their molecules.

There is a third argument noticed by Dr. Prout, to which M. Dumas attaches much weight, in support of the doctrine of equality of atoms in a given volume. It is founded on the recent experiments of MM. Delarive and Marcet, which show that all the gases, in equal volumes, have the same capacity

* *Mécanique Céleste,* livre xii. ch. i. tom. v. p. 89—91.

for heat. Now it had long ago been suggested by Dr. Dalton as the most probable view of the relations of elastic fluids to heat, that "the quantity of heat belonging to the ultimate particles of all elastic fluids must be the same under the same pressure and temperature." Dulong and Petit have since inferred from their experiments, that the specific heats of several *simple* bodies in the solid state, when multiplied by their atomic weights, give a constant quantity as their product*. This relation has more recently been shown by M. Neumann to extend to several *compound* mineral substances†. Admitting, then, the equality of the specific heats of the gases when equal volumes are compared, and also that their ultimate atoms possess the same amount of heat, M. Dumas's conclusion, that equal volumes must contain the same number of ultimate atoms, is perfectly legitimate. But one of the elements of his calculation is erroneous‡. M. Dulong, in his elaborate memoir on specific heat§, has subsequently established the impossibility of obtaining, by the experimental process of Delarive and Marcet, even an approximative measure of the specific heats of the different gases, and has shown that the earlier results of Delaroche and Berard are still those most deserving of confidence. His own experiments, founded on the relations between the specific heats of gases and their powers of propagating sound, concur with those of Berard in indicating considerable differences in the specific heats of the gases, whether equal weights or bulks are made the objects of comparison. Substituting, then, these results for those of Delarive, we obtain, by the process of reasoning adopted by Dumas, the opposite conclusion,—that equal volumes of the different gases compared, contain *unequal* numbers of atoms ||.

* *Ann. de Chimie et de Phys.* tom. x. p. 405.

† *Poggendorff's Annalen*, vol. xxiii. p. 32.

‡ *Traité de Chimie appl. aux Arts*, tom. i. p. 41.

§ *Ann. de Chimie et de Phys.* tom. xli. p. 113.

|| This argument is not urged as possessing more than a *negative* force. The specific heats of the gases are not yet determined with certainty; and it is even doubtful whether, if obtained, they would faithfully represent the absolute heats,—a supposition manifestly involved in the principle of calculation, that the specific heats are equal to the absolute heat of one atom multiplied by the number of atoms in a given volume. It cannot, moreover, be denied, that the specific heats of hydrogen, oxygen and nitrogen, obtained by Delaroche, are so nearly the same in equal volumes, that, allowing for probable errors, they may safely be regarded as identical. Hence, upon the principle of Dulong and Petit, those three gases must contain the same number of atoms, and the weight of the atom of oxygen must be represented by 16 (the number adopted by Berzelius) instead of 8.

It has been the object of the foregoing remarks to prove that there does not exist, in the principles of general physics, any foundation for the new doctrine of Dr. Prout and M. Dumas, " that a given volume contains the same number of ultimate atoms in all the different gases*." These principles, however, with the exception of the relations of specific heats, though they do not furnish any support to such a doctrine, must be acknowledged to involve nothing that is contradictory to it. It must be considered, therefore, simply as an hypothesis, the value of which is to be estimated by its applicability to chemical phænomena. When tried by this test, it will be found wholly untenable, unless it be supported by a second and yet more improbable hypothesis, " the divisibility of the atom." Indeed, the single example of muriatic acid gas is sufficient to demonstrate its unsoundness. A volume of this gas is constituted of half a volume of hydrogen and half a volume of chlorine. The number of atoms in a volume of hydrogen is therefore double that in the same volume of muriatic acid gas. Nitrous gas, in like manner, must contain half the number of atoms that are contained in an equal volume of azote. The same is true of ammoniacal gas, of hydriodic acid gas, of hydrocyanic and chlorocyanic acid vapours, and of the vapour of sulphuret of carbon, when compared with an equal volume of one of their constituents. It may, then, be confidently asserted, that chemical phænomena, at least as they are now generally interpreted, are inconsistent with the notion of an equality of atoms in all gases, compound and simple.

It is solely upon this supposed numerical equality of atoms that Dr. Prout's second proposition is founded. Now if it has been shown that such equality is not derivable from physical principles, and is also inconsistent with known chemical facts, that proposition can be no longer maintained, except as an independent hypothesis; and we are compelled, by the rules of philosophizing, to recur to the simple and beautiful conception of the indivisibility of the atom, taught by the illustrious author of the atomic system. Several considerations may, moreover, be urged in favour of the doctrine of Dalton, that the mutually repulsive molecules of elastic fluids are identical

* It is not asserted that there do not exist any two gases which contain in the same volume the same number of ultimate atoms. On the contrary, most of the simple gases and vapours, and some of the compound gases, are generally believed to be thus similarly constituted. We object only to the raising what is true in certain individual examples into a general and *necessary* proposition.

with the ultimate chemical atoms. We have already had occasion to refer to the postulates employed by Laplace as the basis of his profound mathematical inquiries into the constitution of elastic fluids. Now if heat be combined with the particles of matter by a principle of attraction or affinity (as supposed by Laplace), it is impossible to conceive such affinity to be exercised by aggregates of atoms, and yet not to be the attribute of the single atoms, of which such aggregates are composed. And if the ultimate atoms be endowed with an affinity for caloric, no reason can be assigned why union should not take place between caloric and each ultimate atom singly; nor, the molecules of heat being self-repulsive, why the ultimate atoms after such union should not become mutually repulsive. The contrary hypothesis of Dr. Prout involves the anomaly of supposing heat to have a combining affinity for two or more atoms, while it is destitute of such affinity for single atoms; and also that of supposing two atoms to have relations towards two atoms, or three towards three, which do not obtain between single atoms.

It is furthermore apparent, that the question respecting the mode of union between heat and the molecules of bodies, is not limited to the constitution of elastic fluids, but must equally comprehend the conditions of liquid and of solid. Now the relations of several simple bodies to heat, established by the experiments of Dulong and Petit, point unequivocally to the chemical atoms, as determining the measure of specific heat. In the thirteen simple substances which were the subjects of experiment, they found that the product of the specific heats into the atomic weights was invariably a constant quantity, and consequently that the ultimate atoms contained precisely the same quantity of caloric. These results have been since confirmed by various German experimenters*, and can only be reconciled with the doctrine of the combination of heat with the *ultimate chemical atom.*

In recapitulation, it has been shown,

1st, That the law of Mariotte, and that of the equal expansibility of the different gases, are mathematically derived from elements altogether foreign to the numerical relations of their ultimate molecules; and that no corollary is contained in those laws, determining equality of atoms in a given volume of the different elastic fluids.

* See Mr. Johnston's excellent and comprehensive Report on Chemistry, in the *Report of the First and Second Meetings of the British Association for the Advancement of Science*, p. 418.

2nd, That the most trustworthy experiments on the specific heats of the gases, combined with the law of Dalton, Dulong, and Neumann, lead to an opposite inference.

3rd, That several examples of chemical combination are inconsistent with the doctrine of numerical equalityof atoms in equal spaces of the different gases.

4th, That the original hypothesis of Dalton, which contemplates the self-repulsive gaseous molecule as identical with the ultimate chemical atom, has in its favour the greatest amount of probabilities.

Manchester, June 7, 1834.

V. *A Demonstration of the Parallelogram of Forces.* By J. H. Pratt, *Esq.**

LET P Q be two forces acting on a point, their directions including an angle α; and let R be their resultant, acting in the same plane with P and Q, and making an angle θ with P.

Suppose P to be equivalent to two forces P_1 P_2, acting in direction of R, and perpendicular to this direction; and let Q be equivalent to Q_1 Q_2, acting similarly;

$$\therefore \left.\begin{aligned} R &= P_1+Q_1 \\ 0 &= P_2-Q_2 \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (1.)$$

To find the relation between P_1 and P, we observe that since the law of resolution must be independent of the *magnitudes* of P and P_1 so long as their ratio is the same, P_1 must be of the form $\quad P_1 = P.f(\theta),$

where $f(\theta)$ is to be found.

$$\text{Now, when } \theta = 0,\ \frac{\pi}{2},\ \pi,\ \frac{3\pi}{2},\ 2\pi,\ \ldots$$

$$P_1 = P,\ 0,\ -P,\ 0,\ P,\ \ldots$$

and from these we see that if n be any integer

$$f\left(n.\frac{\pi}{2}\right) = \cos\left(n.\frac{\pi}{2}\right) \quad \ldots\ldots\ldots\ldots \quad (2.)$$

When θ has any value not comprised in the above formula, the only conditions to be satisfied by $f(\theta)$ are, that the sum of the resolved parts of P_1 and P_2 in the direction of P shall equal P, and perpendicular to this equal 0:

$$\therefore \quad P = P_1 f(\theta) + P_2 f\left(\frac{\pi}{2} - \theta\right),$$

* Communicated by the Author.

and $$0 = P_1 f\left(\frac{\pi}{2}-\theta\right) - P_2 f(\theta)$$

or $$1 = \overline{f(\theta)}|^2 + \overline{f\left(\frac{\pi}{2}-\theta\right)}|^2 \quad \ldots\ldots\ldots\ldots\ldots (3.)$$

The second equation is identical.

Now any form of $f(\theta)$ which fully satisfies the equations (2.) (3.) must satisfy the conditions of the problem; and *there can be but one form* which does satisfy the equations, for $\frac{P_1}{P}$ must have a definite numerical value for every value of θ, and $\therefore f(\theta)$ can contain no arbitrary quantities.

Now $$f(\theta) = \cos\theta$$

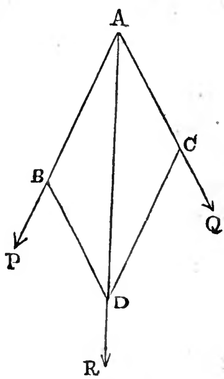
fully satisfies equations (2.) (3.), $\therefore$ *this* is the only, and $\therefore$ the true, value of $f(\theta)$.

Hence by equations (1.)

$$R = P\cos\theta + Q\cos(\alpha-\theta)$$
$$0 = P\sin\theta - Q\sin(\alpha-\theta) \quad \ldots\ldots\ldots\ldots (4.)$$

adding the squares

$$R^2 = P^2 + Q^2 + 2PQ\cos\alpha \quad \ldots\ldots\ldots\ldots (5.)$$



Take AB, AC proportional to P and Q; draw BD parallel to AC, cutting AD in D; and join DC:

$$\therefore BD = \frac{\sin\theta}{\sin(\alpha-\theta)} \cdot AB = \frac{Q}{P} \cdot AB$$

by (4.) = AC.

$\therefore$ AD is in the *direction* of the diagonal.

Also $AD^2 = AB^2 + AC^2 + 2 \cdot AB \cdot AC \cdot \cos\alpha$.

Comparing this with equation (5.), we see that AD represents R *in magnitude* as well as *direction*.

Caius College, Dec. 6, 1833. J. H. Pratt.

VI. *Analysis of* Levyne. *By* Arthur Connel, *Esq. F.R.S.Ed.**

A FEW years ago Sir David Brewster described this mineral as a new species, founding his discrimination of it on an examination of its optical properties by himself, and on a determination of its crystallographical characters by Mr. Hai-

* Communicated by the Author.

dinger*. At the same time he transmitted a small quantity of the mineral to Berzelius for chemical examination; and soon afterwards, in a paper inserted in the Transactions of the Royal Swedish Academy for 1824 †, Berzelius gave the following as the constituents of the mineral:

Silica	48·00
Alumina	20·00
Lime	8·35
Magnesia	0·4
Potash	0·41
Soda	2·75
Water	19·30
	99·21

Berzelius accompanied his analysis with a statement that he considered the mineral to be merely a chabasite. From a subsequent explanation of Sir David Brewster ‡, however, there is every reason to believe that Berzelius had not distinguished the levyne from some chabasite which accompanied it in the same specimen, and that in reality a mixture of both minerals had been subjected to analysis, a mistake which is not very surprising, considering that levyne was at that time entirely a new mineral, and its aspect consequently little known.

Had not a reasonable doubt been thus thrown on the identity of the subject of Berzelius's analysis, any further examination of the chemical nature of levyne would have been a very superfluous task; but under the circumstances, it became very desirable that a new analysis should be executed of undoubted crystals of this mineral.

The specimens examined were partly furnished me by the kindness of my friend Mr. Robert Allan, and partly in my own possession. They were all of Irish locality §. The crystalline form was quite distinct, and exactly that described by Mr. Haidinger. I regret that it was not in my power to operate on larger quantities of materials than those employed.

To determine the specific gravity of the mineral, the largest portions of the crystals separated were selected, the quantity so selected amounting to only 9·3 grains. Their specific gravity was found to be 2·198 at 55° Fahr. The mineral by ignition gave off water, the proportion of which in a mean of two trials, giving almost exactly the same result, amounted to 19·51 per cent.

To determine the proportion of the other constituents, 10·28 grains of the crystals in impalpable powder were treated

* Edinburgh Journal of Science, vol. ii. p. 332. † p. 356.

‡ Edinburgh Journal of Science, vol. iv. p. 316.

§ In addition to the other known localities of this mineral, I may mention that I have found specimens of it in the island of Skye.

with muriatic acid. Speedy gelatinization took place with rise of temperature. Silica was separated in the usual manner, and, after ignition, weighed 4·71 grains. From the residual liquid ammonia threw down alumina, which when ignited weighed 2·4 grains. By dissolving it in muriatic acid, and by the subsequent action of caustic potash, and other necessary steps, there were separated from it ·03 of silica, ·03 of oxide of iron, and ·03 of red oxide of manganese, equivalent to ·027 of protoxide, leaving 2·31 for the amount of the alumina. To the liquid which had been precipitated by ammonia, oxalate of ammonia was added, its action being aided by heat. The precipitate obtained yielded by calcination under the usual precautions 1·84 of carbonate of lime, from which, however, ·05 of oxide of iron were separated by solution in diluted muriatic acid, and the agency of hydrosulphuret of ammonia, leaving 1·785 of carbonate of lime, equivalent to 1·004 of lime. The remaining liquid was evaporated to dryness, and the ammoniacal salt driven off by heat, when a residue of ·61 remained, which, when dissolved in water, left ·02 of silica, with a trace of lime and magnesia. The solution gave cubical crystals by spontaneous evaporation; and by the agency of chloride of platinum and of alcohol, these crystals were found to consist of ·20 chloride of potassium, equivalent to ·13 potash, and ·39 chloride of sodium, equivalent to ·16 soda.

We have thus in 10·28 grains of the mineral, exclusive of water,

Silica (4·71 + ·03 + ·02)	4·76
Alumina	2·31
Lime	1·
Soda	·16
Potash	·13
Oxide of iron (·03 + ·05)	·08
——— manganese	·02
	8·46

And in 100 parts:

Silica	46·30	contains oxygen ...	24·05	... 7
Alumina	22·47	————	... 10·49	... 3
Lime	9·72	————	2·73	
Soda	1·55	————	·39	
Potash	1·26	————	·21	
			—— 3·33	... 1
Oxide of iron	·77			
——— manganese	·19.			
Water	19·51	————	17·23	... 5
	101·77*			

* In operating upon small quantities, any accidental loss or excess of course becomes magnified when reduced to 100 parts. The excess on the analysis of 10·28, taking the proper proportion of water into account, was only ·18.

For the purpose of comparing this analysis with the constitution of chabasite, we may take the following examples of the analysis of the ordinary lime chabasites.

Silica	50·65	...	48·38	...	50·14
Alumina	17·90	...	19·28	...	17·48
Lime	9·73	...	8·70	...	8·47
Potash	1·70	...	2·50	...	2·58
Water	19·50	...	20·00	...	20·83
	99·48 *		98·86 †		99·50 ‡

From this comparison it undoubtedly appears that the mineral under examination possesses, chemically speaking, considerable analogy with chabasite, but still the differences seem to be such as prepare us for admitting it as a distinct species, provided that its crystallographical and optical properties lead us to that conclusion.

Had the form of the crystal of levyne been found to be the same as that of chabasite, we might have admitted that the differences of the composition of the two minerals arose from accidental impurities, examples of such discrepancies sometimes occurring in regard to different individuals of the same species, as in the different analyses of barytic harmotome. But according to the determination of Mr. Haidinger, the crystallographical differences between levyne and chabasite are of a very marked description, the fundamental form of the former mineral being a rhomb of 79° 29′, whilst that of the latter is a rhomb of 94° 46′. A like discrepancy occurs in regard to the optical properties as determined by Sir David Brewster, levyne conforming to the general law of rhombohedral crystals in having one axis of double refraction, whilst the optical structure of chabasite is very anomalous.

It would appear, therefore, that we cannot hold chabasite and levyne to be the same mineral without disregarding crystallographical and optical differences of a marked description; and on the other hand, there evidently appear to be sufficient chemical differences to entitle us to give effect to the distinctions of external and optical characters, the difference of composition being at least as great as that between some other well established species, as, for instance, between stilbite and

* From Gustavsberg, analysed by Berzelius. — Edinb. Phil. Journal, vol. vii. p. 10.

† From Fassa, analysed by Arfwedson.—*Ibid.*

‡ From Renfrewshire, analysed by myself.—New Edinb. Phil. Journal, vol. vi. p. 266.

heulandite. The formula which seems best to express the constitution of levyne is

$\left.\begin{matrix}C\\N.\\P\end{matrix}\right\} S+3\,A\,S^3+5\,Aq$, which differs from that usually adopted for chabasite in containing one atom less of silica, and one atom less of water.

VII. *On the probable future Extension of the Coal-fields at present worked in England.* *By the Rev.* W. D. Conybeare, *M.A., F.R.S., &c.*

[Continued from vol. iv. p. 348.]

I HAVE already pursued my proposed examination through a considerable proportion of the coal-fields of our central district (namely, the eastern division, including the fields of Ashby-de-la-Zouch and Warwickshire, and the smaller patches near Ashborne on the south of the great Derbyshire chain). I consider the general result to be, that throughout this central district the whole stratification is so extremely disturbed and undulating that we are scarcely able to form any anticipations as to the probable prolongation of the beds which can be at all relied upon; but that in very few instances the boundaries of the fields have as yet been ascertained with anything like scientific exactness; that a survey undertaken expressly with a view to this inquiry is undoubtedly very desirable; and that it is little to the credit of a nation like ours, so peculiarly dependent on this branch of her mineral resources, that we thus continue contentedly to acquiesce in a state of ignorance so easily removed. We here see a strong instance of our want of a regular school of mining, such as is possessed by many other countries.

In continuing my own imperfect hints with a view to such ulterior inquiries, I shall first complete the central districts by noticing the Dudley coal-field.

Dudley Field.—The boundaries of this field can hardly yet be considered as accurately ascertained. The anticlinal ridge of transition limestone of Dudley throws up the beds which crop out all round it; and as on the eastern edge of the field near Walsall, the same transition limestone again emerges, we may consider the coal-measures around Bilston as lying in a trough between these points. I do not find any account of the exact limits of this trough on the N.W. border from the Dudley limestone range to Cannock, at the northern apex, or on the N.E. from Cannock to Walsall; but I rather believe that the beds

crop out in these directions; so that we cannot in these quarters look for any probable extension. Not so, however, with regard to that portion of the coal-field which, ranging beneath the overlying basalt of the Rowley Hills, extends to the west and south-west of Dudley: here from Wolverhampton to Stourbridge the beds dip beneath the new red sandstone in a westerly direction, and pursuing that course about 10 miles, we see the coal-measures again emerging from beneath this investiture around Over Arley in Shropshire. *The western border of this Dudley field, and the eastern border of corresponding Shropshire fields, ought to be carefully examined, as it seems very probable that the strata may here extend continuously within workable depth.*

Indications of Coal at the foot of the Bromsgrove Lickey.— These are so exceedingly shattered and disturbed as to afford very little prospect of leading into any valuable working districts.

Coal-fields of Northern Staffordshire.—These, which follow in order the patches near Ashborne, mentioned in my last, consist of two detached fields, lying against the S.W. corner of the great Derbyshire Penine chain: 1. the field of Cheadle, ranging along the river Churnot, described by Farey as a detached basin reposing on the millstone grit; and, 2. the Pottery coal-field of Newcastle-under-Line, occupying a triangular area extending to Congleton on the north. From the eastern and western sides the beds dip towards the centre; but we are not informed in what manner they are disposed along the southern base of the triangle by Newcastle: as they are here overlaid by the new red sandstone, their prolongations may very possibly be traced to some distance beneath it. *This portion requires reexamination.*

Great Manchester Coal-field.—This field, reposing against the western slope of the Penine chain, ranges from Macclesfield to the east of Manchester, and then curving to the W. and S.W., extends nearly to Liverpool. The public is not yet in possession of any scientific description of this most important field; but recent announcements promise that this desideratum will be shortly supplied. As this coal-field approaches near to the æstuary of the Mersey, where it dips beneath the new red sandstone, and as along the western border of the almost contiguous æstuary of the Dee the coal-measures again emerge, skirting the whole Flintshire coast of that æstuary, I am persuaded that they will hereafter be found to extend continuously, and within workable depth between these points. *The whole peninsula of Wirral may be expected thus to afford a productive coal-field: this is probably*

the most important accession which we can look for of the fields already worked.

North Welsh and Shropshire Coal-fields.—The memoirs of Mr. Murchison promise to afford important additional information concerning these districts. I have already in this communication indicated their probable eastern extension, viz. of the Flintshire coal-field to join that of Lancashire; and of the Shropshire fields, near Bridgenorth and Over Arley, to join that of Dudley. With regard to the north-western coal-fields, as we approach the Cumbrian mountains, or lake district, that of Ingleton on the south of the carboniferous limestone encircling this group appears to constitute a small basin reposing on millstone grit, the shattered tract extending hence eastwards to Giggleswick; and the immense faults which traverse this district have been admirably described by Mr. Phillips (Geological Transactions, New Series, vol. ii.).

On the northern border of the Cumbrian mountains, from Whitehaven on the western coast, to Ravensworth, where the Cumbrian and Penine chains inosculate, on the east, a regular zone of coal-measures appears to succeed the carboniferous limestone, and though the principal workings are at the two extremities, indications have been found at various points between these fields; hence we may hereafter look for a considerable extension beneath the new red sandstone of the Vale of Eden.

In my next communication I propose to conclude these remarks by a similar notice of the south-western coal district.

Your old Correspondent,

W. D. Conybeare.

VIII. *On the Influence of the Climate of Naples upon the Periods of Vegetation as compared with that of some other Places in Europe.* *By* John Hogg, *Esq., M.A., F.L.S. F.C.P.S., &c.*

[Continued from vol. iv. page 279.]

II. *Frondescence.*

THE time of the unfolding of the leaf-buds of trees, which Linnæus has called *Frondescentia*, and which the French botanists have distinguished by the word *Bourgeonnement*, presents the same variations that have been observed in the germination of seeds: since the diversity of the climates and of the seasons exerts likewise a great influence on this period of vegetation.

In the same places of his *Philosophia Botanica* before men-

tioned, Linnæus relates some observations concerning the *frondescence* of trees in the vicinity of Upsal, in which he there says that the Elder unfolds its buds in the first days of March; the Indian Chestnut, the Pear, the Spindle Tree, open in the beginning of April; the Elm, the Cherry Tree, the Filbert, in the middle of March; the Birch, the Beech, the Lime, and the Oak, in the first days of May.

About Naples, the Elder developes its leaves in the first 15 days of January; the Elm and the Filbert open their buds in the beginning of February; the Spindle Tree, and the Indian Chestnut in the first week in March; the Birch, the Beech, the Lime, about the 15th of the same month; the Hazel and the Oak in the beginning of April.

In general, we may therefore assert, that in the environs of Naples the expansion of the leaf is earlier by one month and a half than that of the same plants in the North of Europe.

Being desirous to compare this time of vegetation of the trees near Naples with that of the trees which grow in the neighbourhood of Paris, I have consulted the observations made by Dr. Chavassieux d'Audibert in his *Exposition des Températures*, and I have found that this period of vegetation gives, between the two countries, the difference of one month.

In fact, M. d'Audibert fixes the middle of February for the appearance of the leaves of the Elder; March, for that of the Osier, of the Elm, of the Almond, and of the Chestnut; April, for that of the Birch, of the Hazel, and of the Bramble; May, for that of the Oak and Mulberry; whilst around Naples, as it has been before shown, these trees put forth their leaves one month sooner.

[Now, in England, we learn from the Naturalist's Calendar, kept from 1768 to 1793, that at Selborne in Hampshire, Mr. White has recorded March 13. as the *earliest*, and March 20. as the *latest* date, in which he noticed the expansion of the leaves of the Elder; but Mr. Markwick observed January 24. and April 22. as the earliest and latest days for the same occurrence at Catsfield near Battle in Sussex. In the same Calendar, it is also stated that the leafing of the Elm was seen on April 3. by White, and on April 2. and May 19. (earliest and latest) by Markwick. That of the Beech, according to White, occurred on April 10. the earliest, and on May 8. the latest, whilst according to Markwick April 24. and May 25. are the soonest and latest days. And for that of the Mulberry with the former, May 27. and June 13., but with the latter, May 20. and June 11. are the earliest and the latest dates given.

By calculating the *mean* day between the earliest and latest

of these observations, we have March 16. (White at Selborne) and March 8. (Markwick at Catsfield), for the frondescence of the Elder. April 3. (W.) and April 25. (M.) for that of the Elm. April 23. (W.) and May 9. (M.) for that of the Beech, and June 4. (W.) and May 31. (M.) for that of the Mulberry.

Again, we find from the table of the "Indications of Spring," published in vol. ii. p. 128, of Loudon's Magazine of Natural History, which contains the result of more than 60 years' (from 1735 to 1800) observation by Mr. Marshham, and Lord Suffield, at Stratton Strawless in Norfolk, that the leafing of some of the before-named trees took place there according to the following dates.

Leafing.	Earliest.	Latest.	Greatest difference observed in	Medium Time.
Elm......	1779. Mar. 4	1784. May 6	47 years—63 days	1773. April 6
Birch....	1750. Feb. 21	1771. May 4	52 years—72 days.	1745. March 29
Beech....	1779. April 5	1771. May 10	53 years—35 days.	1785. April 23
Lime....	1794. Mar. 19	1756. May 7	43 years—49 days.	1796. April 13
Oak......	1750. Mar. 31	1799. May 20	54 years—50 days.	1757. April 26
Chestnut	1794. Mar. 28	1770. May 12	36 years—45 days.	1776. April 21

Hence we may say, from the data before stated, that at Upsal the Elder unfolds its leaves about one week, the Elm one month, sooner; but the Beech from three days to a week, the Oak seven or ten days, the Lime two to three weeks, and the Birch nearly five weeks later than the same usually do in England. That at Naples, the Elm is earlier by about ten weeks, the Elder nine weeks, the Beech seven weeks, the Lime four weeks, the Oak three weeks, and the Birch two weeks, than in England. And that at Paris, the Elder, the Elm, the Chestnut, and the Mulberry are from two to four weeks sooner, but the Birch and Oak are some days later than in England.

However, the different times of leafing of these trees will be more clearly understood from the annexed comparative Table.

Leafing.	Upsal.	Naples.	Paris.	Selborne.	Catsfield.	Stratton.	England.
Elder....	Mar.1—8	Jan. 1—15	Feb. 14	March 16	March 8	......	March 12
Elm......	Mar. 15	Feb.1— 8	March	April 3	April 25	April 6	April 15
Birch....	May 1—8	March 15	April	......	......	March 29	March 29
Beech....	May 1—8	March 15	......	April 23	May 9	April 23	May 1
Lime....	May 1—8	March 15	......	......	......	April 13	April 13
Oak......	May 1—8	April 1—8	May	......	......	April 26	April 26
Chestnut	......	......	March	......	......	April 21	April 21
Mulberry	......	......	May	June 4	May 31	......	June 2

The last column gives the medium between the mean day of White's observations at Selborne, that of Markwick's at Catsfield, and that of Marsham's at Stratton, which may therefore be taken with considerable accuracy for the regular date of the appearance of the leaves of those trees in the southern and eastern divisions of England, and, indeed, in England in general, excepting, perhaps, its extreme northern parts.]

On comparing the times of leafing of the same trees in different years near Naples, it will easily be seen that this period of vegetation varies according to the temperature which has prevailed during the months of January, February, and March. Thus, for example, in the year 1807, these three months having continued very cold, the Elder opened its buds at the beginning of February; the Elm and the Filbert showed their leaves at the end of the same month; the Birch, the Beech, the Oak, and the Lime were not seen in leaf till towards the middle of April. On the contrary, in the year 1808, the same months having been extremely mild, the frondescence of the same trees took place successively fifteen days earlier. Finally, in 1810, the thermometer in the beginning of March having risen to 15° Reaumur (or nearly 66° Fahrenheit), in the course of the same month the leaf-buds of those trees which usually open in April, were observed to have fully expanded themselves.

[Now, in order to ascertain more correctly the true time, in which the leafing of these trees is wont to occur about Naples, let us take the mean dates of the different years given by Tenore, and the results are as follows:

Leafing.	A. as before.	1807.	B. 1808.	Mean of A and B.	True Mean.	England.
Elder...	Jan. 1—15	Feb. 1—8	Jan. 17—24	Jan. 8—20	Jan. 14	March 12
Elm.....	Feb. 1—8	Feb. 21—28	Feb. 6—13	Feb. 4—9	Feb. 6	April 15
Birch...	March 15	April 8—15	Mar. 24—31	Mar. 15—27	March 21	March 29
Beech...	March 15	April 8—15	Mar. 24—31	Mar. 15—27	March 21	May 1
Lime ...	March 15	April 8—15	Mar. 24—31	Mar. 15—27	March 21	April 13
Oak.....	April 1—8	April 8—15	Mar. 24—31	Mar. 27 to Ap. 4	March 31	April 26

Hence, by the above comparison of the *true mean* dates with those in England, we find that near Naples the Elm is earlier by about nine weeks, the Elder eight weeks, the Beech six weeks, the Lime three weeks, the Oak nearly four weeks, and the Birch only one week, than with us.]

But, likewise at Naples, there are not wanting several sorts of trees which are always very late in developing their leaf-buds. I will mention *Acer platanoides* and *A. Lobelli*, which having been transplanted from the high mountains, where

they grew naturally, to the Royal Botanical Garden, annually retain their native slowness* in leafing; in as much as the first does not open its buds until the end of April, and the second keeps them closed to the end of the early part of May. The same circumstance has happened to the Red Lime tree (*Tilia rubra?*) originally brought from Hungary, and which in the Botanic Garden keeps its buds unexpanded till after the beginning of May.

[To be continued.]

IX. *Characters of some undescribed Species of* Araneidæ. *By* JOHN BLACKWALL, *Esq.*, *F.L.S.*, *&c.*†

Tribe, TUBITELÆ, Latreille.

Genus, *Drassus*, Walckenaër.

Drassus cupreus.

CEPHALOTHORAX oval, convex above, thinly covered with fine, short hairs, marked with slight furrows on the sides, and a narrow, longitudinal indentation in the medial line of the posterior region. Eyes disposed in front in two transverse rows somewhat curved, having their convexity directed backwards; the posterior row is rather the longer of the two, the intermediate eyes, which are oval, and nearer to each other than they are to the lateral eyes of the same row, forming a quadrangle with the intermediate eyes of the anterior row. Mandibles strong, conical, armed with a few teeth on the inner surface, and projecting a little forwards. Maxillæ long, convex at the base, underneath, enlarged externally where the palpi are inserted, and at the extremities, which are obliquely truncated on the inner side; they are depressed and contracted in the middle, and curved towards the lip, which is longer than broad, and truncated at the apex. Pectus oval. Legs robust, moderately hairy, and provided with a few sessile spines; the fourth pair is the longest, then the first, the third pair being the shortest. Each tarsus has a climbing apparatus on the under side, and two pectinated claws at its extremity. A single dentated claw terminates each palpus. These parts are of a pale reddish brown colour, a fine line of a blackish hue occurring on the margins of the cephalothorax, and a band of the same tint bordering the pectus and lip. Abdomen oblong oval, thickly covered with short hairs of a bright reddish copper-colour, the under part being the palest; at the anterior extremity, contiguous to the cephalothorax, is a tuft of long, deep black hairs, a band of a blackish hue, broad before and tapering to a point behind, extending from it, along the medial line of the upper side, rather more than half the length of the abdomen. In some specimens this band is not perceptible.

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* This is also the case with the common Birch tree, which, and *Acer platanoides*, according to Linnæus, "habitant in *Europâ frigidiore*." Being natives of a very cold climate, these trees, although growing in warmer countries, as Naples, Paris, &c., still retain in a remarkable manner their naturally late frondescence; therefore, it seems that heat cannot produce such an effect on this period of vegetation, as it has been shown to do on germination.—J. H.

† Communicated by the Author.

Plates of the spiracles large and of a pale yellow colour. Spinning mammulæ prominent and cylindrical, the inferior pair appearing to be the longest when in a state of repose.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{2}{5}$ths of an inch; length of the cephalothorax $\frac{1}{6}$; breadth $\frac{1}{8}$; breadth of the abdomen $\frac{1}{8}$; length of a posterior leg $\frac{1}{2}$; length of a leg of the third pair $\frac{2}{5}$.

The male resembles the female in colour, but as the fifth or terminal joint of the palpi, in all those individuals which have fallen under my observation, has been ovate in figure and simple in structure, it is evident that they had not attained maturity.

This species, which has a close affinity with *Drassus sylvestris*, I have found inclosed in white, silken tubes, of a fine, compact texture, attached to the inferior surface of stones and fragments of rock, in the neighbourhood of Manchester, and near Llanrwst in Denbighshire.

Tribe, Inequitelæ, Latreille.

Genus, *Theridion*, Walckenaër.

Theridion riparium.

Cephalothorax inversely heart-shaped, convex and glossy above, with a large indentation in the medial line of the posterior region. Mandibles small, conical, perpendicular. Maxillæ obliquely truncated on the outer side at the extremity, and inclined towards the lip which is quadrate. Pectus heart-shaped. Palpi short and robust. These parts are of a red-brown colour, the cephalothorax and pectus being much the darkest. Legs strong; the first pair is the longest, then the fourth, the third pair being the shortest; they are of a yellowish brown colour, with broad bands of red-brown. Eyes situated on the anterior part of the cephalothorax; four, which are intermediate, form a square, the two in front being seated on a protuberance; the other four are disposed in pairs on the sides of the square; the eyes constituting each pair are placed obliquely on an eminence and are contiguous. The abdomen, which is thinly covered with short hairs, is remarkably convex, projecting over the base of the cephalothorax; it is red-brown above mottled with black and white, and is bisected by an irregular, transverse, white line, interrupted in the middle by a triangular, black spot, between which and the spinners is a curved, transverse, black line; under side of the abdomen brownish black with a transverse band of a red-brown colour near the spinners. Plates of the spiracles red-brown.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{7}$th of an inch; length of the cephalothorax $\frac{1}{14}$; breadth $\frac{1}{20}$; breadth of the abdomen $\frac{1}{10}$; length of an anterior leg $\frac{3}{10}$; length of a leg of the third pair $\frac{3}{20}$.

The female of this species spins an irregular web of fine glossy lines under the projections of broken, precipitous banks, in the woods about Oakland. In the month of August she constructs a long, slender, conical, upright tube of silk, of a slight texture, measuring from one and a half to two and a half inches in length, and about half an inch in diameter at the lower extremity; it is closed above, open below, and thickly covered on the outside with bits of earth, minute pebbles, dried leaves, flowers of heath, &c. Suspended from the projection of the bank to which the web is attached by strong lines connected with the apex, and united to the web laterally by numerous slender threads, the tube is held firmly in its position. In the upper part of this curious domicile the spider fabricates two or three slight, globular cocoons of yellowish white silk, about $\frac{1}{10}$th of an inch in

diameter, in each of which she deposits from ten to thirty spherical eggs of a yellowish white colour. The young continue with the mother till they have attained a considerable size, and are provided by her with prey, as the contents of the tube plainly indicate. I have not succeeded in capturing an adult male of this species of *Theridion*,

Genus, *Neriene*, mihi.

Neriene nigra.

Cephalothorax inversely heart-shaped, inclining to oval, convex above, glossy, marked on the sides with slight furrows diverging from the superior part towards the lateral margins, prominent in front, depressed in the posterior region, with an indentation in the medial line. The intermediate eyes of the anterior row are very minute and near to each other. Mandibles strong, conical, armed with teeth on the inner surface, and slightly inclined towards the pectus, which is heart-shaped. Maxillæ enlarged at the extremity, and inclined towards the lip, which is semicircular and prominent at the apex. These parts are of a brownish black colour, the mandibles and maxillæ having a faint tinge of red. The legs and palpi are provided with hairs and a few delicate spines, and are of a red-brown colour. Each tarsus has three claws at its extremity; the two superior ones are minutely dentated, and the inferior one is inflected near its base. Abdomen oval, convex above, projecting over the base of the cephalothorax; it is thinly clad with hair, glossy, and brownish black. Plates of the spiracles of a brown colour.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{10}$th of an inch; length of the cephalothorax $\frac{1}{20}$; breadth $\frac{1}{23}$; breadth of the abdomen $\frac{1}{20}$; length of an anterior leg $\frac{1}{8}$; length of a leg of the third pair $\frac{1}{10}$.

The male resembles the female in colour, with the exception of its legs, which are redder. The relative length of the organs of progression is the same in both sexes. The third joint of the palpi is long and clavate; the fourth is strong, and is elongated before into a narrow, oval process tapering to a point, which extends in front of the fifth joint; this latter joint is oval, convex externally, concave within, comprising the sexual organs; they are highly developed, complex with spiny processes, one of which, on the outer side near the extremity, is curved into a circular form, and are of a dark red-brown colour.

Specimens of this spider were captured in the autumn of 1833, on posts and rails at Oakland, and at Crumpsall Hall.

Neriene pygmæa.

Cephalothorax oval, glossy, convex above, with the sides somewhat depressed, and a small indentation in the medial line of the posterior region. Mandibles strong, conical, armed with teeth on the inner surface, and slightly inclined towards the pectus, which is heart-shaped. Maxillæ enlarged at the extremity, and inclined towards the lip, which is semicircular and prominent at the apex. The cephalothorax, pectus, and lip are brown-black, the mandibles and maxillæ being of a dark reddish brown colour. The legs and palpi are provided with hairs and fine spines, and their colour is bright rufous. Each tarsus has three claws at its extremity; the two superior ones are minutely dentated, and the inferior one is inflected at its base. Abdomen oval, projecting a little over the base of the cephalothorax; it is sparingly clad with hair, glossy, and brownish black. Plates of the spiracles brown.

Length, from the anterior part of the cephalothorax to the extremity of

the abdomen, $\frac{1}{11}$th of an inch; length of the cephalothorax $\frac{1}{20}$; breadth $\frac{1}{28}$; breadth of the abdomen $\frac{1}{25}$; length of an anterior leg $\frac{1}{8}$: length of a leg of the third pair $\frac{1}{10}$.

The male resembles the female in colour, and the relative length of its legs is the same as in that sex. The third and fourth joints of the palpi are short, the latter being much the stronger, and prominent in front; the fifth joint is oval, convex externally, concave within, comprising the sexual organs, which are highly developed, complicated in structure, and of a dark reddish brown colour.

I found this minute species in considerable abundance in the month of March 1833, on iron rails at Crumpsall Hall; and in the autumn of the same year I procured specimens in the vicinity of Llanrwst.

Tribe, ORBITELÆ, } Latreille.
Genus, *Linyphia*, }

Linyphia pusilla.

As this spider bears a striking resemblance to *Linyphia minuta*, it will suffice to point out those particulars in which it differs from that species. It is smaller, of a more slender form, and the colour of the legs and palpi is plain, yellowish brown. The upper part of the abdomen is pale brown, and along the middle extends a series of strongly marked, brownish black, angular lines, having their vertices directed forwards; the sides and under part are dark brownish black, and the plates of the spiracles are of a brown colour. The female has no cylindrical appendage in connexion with the sexual organs.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{11}$th of an inch; length of the cephalothorax $\frac{1}{25}$; breadth $\frac{1}{28}$; breadth of the abdomen $\frac{1}{20}$; length of an anterior leg $\frac{1}{6}$; length of a leg of the third pair $\frac{1}{8}$.

The abdomen of the male is more slender, and darker coloured than that of the female, but the relative length of its legs is the same; their absolute length, however, is greater, an anterior one measuring $\frac{1}{4}$th of an inch. The third and fourth joints of the palpi are short, the latter being very strong, and prominent in front; the fifth joint is convex externally, concave within, comprising the sexual organs, which are highly developed, complicated in structure, and of a red-brown colour.

This species is common in autumn on rails in the vicinity of Manchester, and in the neighbourhood of Llanrwst.

Crumpsall Hall, Feb. 10, 1834.

X. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

Mr. Greenough's Anniversary Address.

[Continued from vol. iv. p. 454.]

AMONG the subjects which have for some years past engaged the thoughts of geologists, none perhaps has excited so general and intense an interest as the Theory of Elevation. I shall avail myself, therefore, of the present occasion to lay before you a connected statement of the scattered facts and opinions upon which it rests.

On entering upon this subject, it is necessary to understand distinctly what is meant by Elevation. Definitions have recently been decried, I think unwisely. The formation of definitions, it has been said, and the establishment of unerring distinctions

are among the last, and not the first steps of systematic knowledge. Equally true, and far more salutary is the lesson that science cannot be advanced by equivocation. As in trading concerns fixed weights and measures are necessary guards against fraud, so in philosophical investigation words of definite meaning are indispensable securities against sophistry and self-delusion. Euclid did not end, he began with defining. Mathematical certainty has no other basis than mathematical precision, and the greater part of those absurdities which from time to time attach themselves to all other branches of knowledge derive their subsistence from ambiguity of language and a dearth of definition.

A torrent brings down a quantity of alluvial matter, and the plain on which it rests is said to be *elevated.*

An opening occurs in the earth; ejected ashes, scoriæ and lava accumulate around it; a Monte Nuovo is formed; and the area it occupies is said to be *elevated.*

By the persevering labour of polypi, a coral reef gradually attains the surface of the ocean; and the fabric so constructed is said to be *elevated.*

A porous rock covers a rock that is not porous; the rain filters through the superincumbent bed; springs break out in the subjacent; and at last, for want of support, the porous rock, originally horizontal, acquires an inclined posture, one end being directed upwards, the other downwards; and the whole is said to be *elevated.*

An earthquake takes place at the mouth of a river; the sea is violently affected; a bar is formed at the entrance of a harbour from the washing in of new alluvion, or from some obstruction to the escape of the old; where a ship floated, a barge is aground; and the land is said to be *elevated.*

Such instances of Elevation are common and incontestible; but elevation of this kind is quite different from that which forms the subject of my present inquiry.

By the term *Elevation,* I mean only the removal of any given object from a lower level to a higher level; consequently it is necessary, before I speak of an object as *elevated,* that I should be prepared to show two things: first, the level at which it has stood; secondly, the level at which it stands.

That I might form a right opinion of the theory, the merits of which I am about to investigate, I have endeavoured to determine the site, the number and the magnitude of those multifarious objects to which the attribute of elevation is continually applied. The attempt has proved unsuccessful: they are indefinite in place, in form, and in dimension. That Mountains should be elevated is not surprising, but we are familiarized also with Valleys of elevation*. In ancient times an Island (Delos, for example,) would alternately

* Valleys of this nature are properly called by Mr. Scrope "valleys of "elevation and subsidence," or more concisely, "anticlinal valleys." See Scrope on Volcanoes, p. 213.

emerge from, and plunge beneath, the sea. Extensive Provinces, nay, entire Kingdoms, now perform the same feat. The existence of Craters of Elevation is by some still considered doubtful; but it is an accredited fact that Mountains and Mountain Chains have risen, either *per saltum* or *per gradus*. All the Strata have been raised; and all Unstratified Rocks would doubtless have been raised also, but that some have risen of themselves. The Bed of the Sea has been elevated again and again. Continents too have been raised, though "by an operation distinct from that which raised the Primary Strata."

The arguments advanced in favour of these doctrines are derived either from observation, or from induction.

It is stated by Von Hoff, that in the year 1771 several tracts of land were upraised in Java, and that a new bank made its appearance opposite the mouth of the river Batavia. The authorities cited for the effect of this and several other earthquakes mentioned in the same place by this author, are Sir Stamford Raffles, John Prior's Voyage in the Indian Seas, and Hist. Gen. des Voy. tom. ii. p. 401. Mr. Lyell has cited the first of these only, but no such fact is noted in either edition of the work of Sir Stamford Raffles. The other authorities adduced by Von Hoff I have been unable to consult; but from the Appendix to the Batavian Transactions (which contains an apparently authentic account of all the recorded earthquakes that have taken place in Java during a century and a half,) it would seem, that in the year 1771, in which the uprising is said to have happened in that island, there was no earthquake at all.

The Earthquake of Chili in 1822 has been so much* insisted on, that it requires detailed consideration. Of this event an account by Mrs. Graham is inserted in our Transactions. I am deeply sensible of the honour that lady conferred on the Society by her obliging compliance with the request which elicited her narrative, and it is only the importance of its contents which could induce me to subject them to the test of rigid examination.

According to this account "it appeared on the morning after the "earthquake, that the whole line of coast from north to south, to the "distance of above 100 miles, had been *raised* above its former level." But by what standard was the former level ascertained? who on the morrow of so fearful a catastrophe could command sufficient leisure and calmness to determine and compute a series of changes, which extended 100 miles in length, and embraced (according to a statement in the Journal of Science,) an estimated area of 100,000 square miles? How could a range of country so extensive be surveyed while the ground was still rocking, which it continued to do on that day, and for several successive months? What was the average number of observations per square mile? Who made, checked and registered them? By what means did the surveyors acquaint themselves with what had been the levels and contour before the

* Bakewell's Geology, edition 4, pp. 98. 504. Lyell, vol. i. pp. 401. 455. De la Beche's Manual, edition 2. Scrope on Volcanoes, p. 209.

catastrophe took place, by which, as we are told, all the landmarks were removed, and the soundings at sea completely changed?

Mrs. Graham states that by the dislodgement of snow from the mountains, and the consequent swelling of rivers and lakes, much detritus was brought to the coast; and further, that sand and mud were brought up through cracks to the surface. Amid so many agents it should not be easy to assign to each, its share in the general result.

That fishes lay dead on the shore may prove only that there had been a storm. In her published travels, Mrs. Graham represents them as lying on the beach, which may very well have been thrown up, as the Chesil bank has been, by a violent sea. Some muscles, oysters, &c., still adhered, she says, to the rocks on which they grew; but we know not the nature or dimensions of these rocks, whether fixed or drifted. The occurrence of a shelly beach above the actual sea-level is an observation which must not be lost sight of. I propose to speak of it hereafter: in the mean time be it recollected, that these beaches are said to occur along the shore at *various* heights, along the summit of the highest hills, and even among the Andes.

Neither in the paper of Mrs. Graham, nor in the anonymous account published about the same time in the Journal of Science, can I find any paragraph to justify the position (which, from the seductive character of the work* in which it appears, may, if not now assailed, soon be deemed unassailable,) that a district in Chili, one hundred thousand miles in area, "was *uplifted* to the average height "of a foot or more; and the cubic contents of the *Granitic Mass* "added in a few hours to the land." By what means we get the average I do not know. Mrs. Graham says the alteration of level at Valparaiso, was about three feet; at Quintero, about four feet: but *the granitic Mass!* has the geological structure of Chili been sufficiently examined to assure us that Granite extends over one hundred thousand square miles?

In the well-known work of Molina, a Jesuit who passed the greater part of his life in Chili, and wrote a natural history of that country, I find no ground for supposing that in any earthquakes which took place there from the time the Spaniards first landed on its shores to the date of his publication, any similar phenomena had been noticed. Moreover, the statement of Mrs. Graham, and of the writer before alluded to, respecting the *Elevation of land* which occurred during the earthquake of 1822, has not been confirmed by Captain King, nor by any naval officer or naturalist who has since visited that region, though many have visited it who had heard the circumstance, and who would willingly have corroborated it if they could. But they saw no traces of such an event; and the natives with whom they conversed, neither recollected nor could be induced to believe it.

The 16th number of the "*Mercurio Chileno*," a scientific Journal, contains an account of this earthquake, by Don Camilo Enriquez, which I have not been able to procure. A later number refers to this

* Lyell, vol. i. p. 473.

account, and to another published in the *Abeija Argentina*, a work of considerable reputation, which, by the kindness of Mr. Woodbine Parish, I have been enabled to consult. The account there given of the earthquake of 1822, is strongly recommended to the reader, "as a sensible straight-forward description of what actually took "place, without the high colouring in which ignorance and terror "and exaggeration are apt to indulge."

No notice is here taken of the permanent *Elevation of the Land*, and the account concludes thus:

"The earth certainly cracked in places that were sandy or "marshy; I saw cracks too in some of the hills, but mostly in "the low nook where much earth had run together; the sea was "not much altered,—it retired a little, but came back to its old "place. Don Onofri Bunster, who, on the night of the earthquake, "was walking on the shore at Valparaiso, in front of his house, had "a mind to go up on the hill, but could not, so great was the quan-"tity of falling dust and stones: he repaired to his boat therefore, "and with some difficulty got aboard; this done, he made obser-"vations on the motion of the sea; on sounding, the depth was "thirteen fathoms; he heaved the lead a second time, and the "depth was no more than eight fathoms: this alternate ebbing and "flowing lasted the whole night, *but did not the slightest harm on "shore.*"

These are the only cases I remember to have met with, in which the testimony of eye-witnesses has been adduced to prove the Rise of land by Earthquakes. That such Rise may have taken place, at different times, without being recorded, perhaps even without being observed, is not very improbable; but if I am to pronounce a verdict according to the evidence, I believe there is not as yet one well authenticated instance in any part of the world, of a non-volcanic Rock having been seen to rise above its natural level in consequence of an Earthquake.

Before I quit this subject, it may not be amiss to mention, that on comparing the times at which the successive shocks took place in Chili, as given by Mrs. Graham, and the other authorities to which I have had occasion to refer, the discrepancy is extraordinary.

I have already intimated in a few words, my opinion as to the sense in which land can be said *to be elevated by means of Volcanoes.* Of these, Vesuvius is perhaps the most constantly observed; and among the innumerable authors who have described its effects, from the time of Pliny down to the present day, not one pretends that the Apennine limestone, close at hand, has been in the least raised by that volcano. We shall do well to bear this in mind, when we have occasion to consider the height at which tertiary shells are found on Etna. That those shells belong to beds thrown up by Etna, is a doctrine founded upon induction, not upon experience. As far as experience goes, we have no reason to think that Etna, in its most violent paroxysms, will ever raise those tertiary strata above their present level.

Leaving these scenes of paroxysmal violence, let us next inquire, whether there may not be going on, in the calmest seasons and in the stillest countries, a *chronic and almost imperceptible impulsion of land upwards.*

As early as the time of Swedenborg, who wrote in 1715, it was observed that the level of the Baltic and German Ocean was on the decline. About the middle of the last century an animated and long-continued discussion took place in Sweden, first as to the cause of this phenomenon, and then as to its reality. Hellant, of Tornea, who had been assured of the fact by his father, an old boatman, and who afterwards witnessed it himself, bequeathed all he had to the Academy of Sciences, on condition that they should proceed with the investigation: the sum was small, but the bequest answered the purpose. Some of the members of the Academy made marks on exposed cliffs and in sheltered bays, recording the day on which the marks were made, and their then height above the water. The Baltic affords great facility to those who conduct such experiments, as there is no tide, nor any other circumstance to affect its level, except unequal pressure of the atmosphere on its surface and on that of the ocean: this produces a variation which is curiously exemplified at Lake Malar near Stockholm. As the barometer rises or falls, the Baltic will flow into the lake, or the lake into the Baltic. The variation resulting from the inequality of atmospheric pressure, however, is trifling. In sheltered spots, mosses and lichens grow down to the water's edge, and thus form a natural register of its level. Upon this line of vegetation marks were fixed, which now stand in many places two feet above the surface of the water.

In the year 1820–1, Bruncrona visited the old marks, measured the height of each above the line of vegetation, fixed new marks, and made a Report to the Academy. With this Report has been published an Appendix by Halestrom, containing an Account of Measurements made by himself and others along the coast of Bothnia. From these documents it would appear, 1. That along the whole Coast of the Baltic the water is lower in respect to the land than it used to be. 2. That the amount of variation is not uniform. Hence it follows, that either the Sea and Land have both undergone a *change of level*, or the Land only; a change of level in the Sea only will not explain the phenomena.

A quarter of a century has now elapsed since Mr. von Buch declared his conviction that the surface of Sweden was slowly rising all the way from Frederickshall to Abo, and added that the Rise might probably extend into Russia. Of the truth of that doctrine the presumption is so strong, as to demand, that similar experiments and observations should be instituted and continued for a series of years in other countries, with a view to determine whether any change of level is slowly taking place in those also. The British Association for the Advancement of Science have already obeyed the call. A committee has been appointed to procure satisfactory data to determine this question as far as relates to the coasts of Great Britain and Ire-

land, and I cannot but hope that similar investigations will also be set on foot along the coasts of France and Italy, and eventually be extended to many of our colonial possessions.

The inductive arguments in favour of the *Elevation of land*, whatever the size, and whatever the amount of Rise, are founded chiefly on the following circumstances: 1. The height of sedimentary beds and marine bodies, whether corresponding or not to those of adjacent seas, or of the actual globe. 2. The height of terraces resembling sea beaches. 3. The height of ripple marks. 4. The change of posture which horizontal strata undergo in the neighbourhood of "unstratified rocks." 5. The various heights at which the same rocks occur in different parts of their course. 6. The anticlinical posture of strata frequent in, though not confined to, mountain chains. 7. The arched or domed configuration of some strata. 8. The occurrence of coral, apparently recent, high above the present surface of the sea. 9. The position of ancient buildings, viz. the temple of Serapis at Puzzoli, &c. I have not time to consider these arguments in detail; each deserves to form the subject of a separate treatise. Some of them prove not Elevation, but only change of level, which Subsidence would explain equally well. Some prove local disturbance, whereby one portion may have been thrown up, the other down. Some again afford a fair presumption of real *local* Elevation or Ascent. Most of them are good to a certain point: all are continually overstrained; and I am frequently astonished to observe how prodigious the weight, how slender the string that supports it.

The assigned *Causes of Elevation* are exceedingly various. One author raises the bottom of the sea by earthquakes; another, by subterranean fire; another, by aqueous vapour; another, by the contact of water with the metallic bases of the earth and alkalis. Heim ascribes it to gas; Playfair, to expansive force acting from beneath; Necker de Saussure connects it with magnetism; Wrede, with a slow continuous change in the position of the axis of the earth; Leslie figured to himself a stratum of concentrated atmospheric air under the ocean, to be applied, I suppose, to the same purpose.

It is impossible within the narrow limits of this discourse, that I can enter into the merits of these and other hypotheses seriatim. I must therefore throw them into two classes, the first of explosive forces, the second of sustaining forces; they are one and the same in Plutonic language, but still it will be convenient to separate them.

That explosive forces exist, or may exist, under the surface, no one can deny; but I cannot adopt the opinion (however high the authority from which it comes,) that "in volcanic eruptions we find a power "competent to raise *Continents* out of the ocean." The force we find in volcanic eruptions is limited in time, place and action; it fuses bodies of easy fusibility; it tosses up those that are refractory, and thus forms either a current of lava or a shower of stones, scoriæ and ashes. What resemblance is there between this operation and the rise of a continent? With more propriety might it have been said that in a molehill we behold the action of a cause competent to raise mountains.

If by *Continent* is meant a whole Continent, and nothing but a Continent, its rise, provided this happened only once, would seem difficult to understand; but to me still more incomprehensible is the confident assurance we continually receive from writers of high and deserved reputation, that this event has happened again and again. Before we admit the Submersion of a continent, we must admit either that at a period immediately preceding that catastrophe, there existed under the land a cavity large enough to contain the continent about to be submerged, or that during the process the subjacent beds shrunk in consequence of a reduction of the temperature, and to such an extent that the contraction in a vertical line equalled the distance from the level of the highest tops of the continent to that of the surrounding ocean. In like manner, before we can admit the Elevation of a continent, we must admit either that, at a period immediately preceding that catastrophe, there happened an inroad of sustaining matter equal in thickness and in extent to the Continent about to be uplifted, or that during the process the subjacent beds expanded in consequence of an increase of temperature, and to such an extent that the expansion in a vertical line equalled the distance from the level of the highest tops of the continent to that of the surrounding ocean. These therefore are the events which we are taught to credit, as having taken place again and again, notwithstanding the tendency which caloric has to diffuse itself, and the apparently unaltered dimensions of the fissures and local caverns by which the strata are so often separated or intersected.

I will not expend more of your time in arguing against such doctrines. All men are more or less lovers of the marvellous, but few, I think, will upon reflection approve such marvels as these.

Solids, fluids and aeriform substances exist, we know, in the interior of the earth, and expand by heat, which exists there likewise. All of these, therefore, are fit *Agents of Elevation*, subject to certain conditions.

Dr. Daubeny attributes the liquefaction of lava, the throwing up of ashes, and all other phenomena of disturbance attendant on volcanic eruptions, to the Action of Water upon the Metallic Bases. This cause is not opposed to experience, and appears well proportioned to the effect, which is sudden, violent, occasional, temporary, accompanied by heat and by flame. To me, at least, it seems far more satisfactory than the explanation of those who ascribe the effect to the Elastic Power of Subterranean Fires, repressed in one place and relieved in another, or to the Undulations of a Heated Nucleus.

A heated *Central Nucleus* is a mere invention of fancy, traceable, I believe, to no other source than the hope of obtaining a good argument from the multiplication of bad ones. To the Huttonian and every other geological sectary who relies on this postulate, I say, be cautious; "*incedis per ignes dolosos.*"

The only observation I recollect to have met with in favour of central heat is, that the deepest mines are the warmest—be it so! Might not a geologist by parity of reasoning argue thus?—In travelling from Rome to Chamonix, the country becomes continually more and more mountainous; some of the peaks of Chamonix are from ten to

fifteen thousand feet above the level of the sea. Imagine, therefore, what they must be at Hamburgh!!

If mines derive their temperature from heat lodged in the centre of the earth, the temperature ought to vary with their distance from the centre, and therefore, since the earth is an oblate spheroid, the mines of Scandinavia ought at the same depth from the surface to be proportionally warmer than those of tropical countries; a result which has never been, I believe, even suspected.

The existence of *Central Heat* in the sense and to the extent assumed in the Huttonian theory, is contrary to all our experience. If Heat there be in the Centre of the globe, it must have the properties of heat and none other. I ask not how the Heat originally was lodged in that situation, for the origin of all things is obscure; but I ask why, in the countless succession of ages which the Huttonian requires, the Heat has not passed away by conduction, and if it has passed away, by what other heat it has been replaced?

Dr. Chalmers in speaking of Sir Isaac Newton, observes, that it was a " distinguishing and characteristic feature of his great mind, that it " kept a tenacious hold of every position which had proof to substan- " tiate it; but a more leading peculiarity was, that it put a most de- " termined exclusion on every position destitute of such proof. The " strength and soundness of Newton's philosophy was evinced as much " by his decision on those doctrines of science which he rejected, as " by his demonstration of those doctrines of science which he was " the first to propose. He expatiated in a lofty region, where he met " with much to solicit his fancy, and tempt him to devious speculation. " He might easily have found amusement in intellectual pictures, he " might easily have palmed loose and confident plausibilities of his " own on the world. But no, he kept by his demonstrations, his mea- " surements, and his proofs."

Gentlemen, let us, as far as is consistent with the nature of geological investigation, show the strength and soundness of our philosophy in the same manner.

That Heat of considerable intensity prevails occasionally, in certain places, at some depth, is all that we have as yet clearly established. Whether that Heat is permanent, whether it is generally diffused, whether it is central, are questions of mere speculation.

Intimately connected with the hypothesis of *Central Heat* is that of *Refrigeration.*

It has been observed by one of our members, that " the Remains both " of the animal and vegetable kingdom preserved in strata of different " ages, indicate that there has been a great Diminution of Tempera- " ture throughout the northern hemisphere, in the latitudes now oc- " cupied by Europe, Asia and America; the change has extended to " the arctic circle as well as to the temperate zone; the heat and " humidity of the air, and the uniformity of climate, appear to have " been most remarkable when the oldest strata hitherto discovered " were formed. The approximation of a climate similar to that now " enjoyed in these latitudes, does not commence till the æra of the " formations termed tertiary; and while the different tertiary rocks

"were deposited in succession, the Temperature seems to have been "still further lowered, and to have continued to diminish gradually "even after the appearance of a great portion of existing species upon "the earth." The little knowledge we have of the fossil productions of countries south of the temperate zone, induces me to believe that these observations are as applicable to the southern hemisphere as to the northern.

This *Refrigeration,* one of the most undoubted facts in geology, is supposed by the Huttonians, and if I mistake not, by M. Elie de Beaumont and others, to arise from a decrease of the *Central Heat*; an opinion, however, which cannot, I think, be supported.

We know of one method only by which Central Heat, if it exists, can pass from the earth, viz. by Radiation. It cannot pass by Conduction. Conduction implies conductors, which in empty space are not to be procured *, but the Radiation of heat, at low temperatures, is so slight that it is scarcely sensible at 100° of Fahrenheit's thermometer, a temperature twice as great as the medium temperature of the surface of the globe at this time. The Temperature of the earth's surface has been shown by Fourier to be as constant as are the dimensions of its orbit, and the period of its annual revolution. Laplace observes, that our planet has undergone no Contraction of Size during the last 2000 years; consequently there has been no sensible *Refrigeration* during that period, and the last Seculum of M. de Beaumont has already extended to more than twice the length of a Millennium.

Another argument, or rather postulate, has been adduced in favour of *Central Heat,*—the Fusion of Unstratified Rocks, and their forcible Injection into the Stratified.

Gentlemen, I have confessed to you again and again, that I am not aware, nor has any one as yet informed me, by what test Stratified and Unstratified rocks can be distinguished; the only test I know is the good will and pleasure of those who make the distinction. The followers of Pluto seize and appropriate to his use as many rocks as they think proper. By virtue of such seizure, these Rocks become necessarily Unstratified: why so? because if Stratified they would be no longer Plutonic. Stratification I know is a question to be determined not by the senses but by the fancy; otherwise, I would say, that the magnificent range of basaltic cliff, which extends from the county of Derry along the coast of Antrim as far as Fairhead, is as distinctly stratified as any mountain-limestone, oolite or chalk in Great Britain.

However, I waive this objection as it leads me away from my subject, and return to the consideration of *Central Heat.* Have those who believe in this agent ever taken into their account the nature of the substances said to have been fused? Many of the trap rocks, not all of them, (for the family is large, and many of its members have been introduced into it, not by nature but by adoption,) I attribute to the agency of the causes which have produced lava, causes which, comparatively speaking, I do not believe to be very deep-seated.

* See Comparative View of the Huttonian and Neptunian Systems of Geology.

These rocks I put out of consideration for the present; the remarks about to be offered apply to granite and its congeners, under which head I would give to every one full liberty to include or reject quartz rock, gneiss, mica slate, eurite, cipollino, hornblende rock, serpentine, &c. Some or all of these, it is the bounden duty of *Central Heat* to fuse and to eject.

Such and so limited are the means of Chemistry, that of many substances thus brought within the sphere of our inquiries, the point of fusion is at this day unascertained. The author of the masterly publication before adverted to, brought together many useful observations upon this subject. He observes that "Lavoisier could not melt a "particle of Carbonate of Lime by the intense heat of a burning mir-"ror, and that Quartz, according to Saussure, requires for its fusion "a temperature = 4043° of Wedgwood's pyrometer, Glass requiring "at a medium only 30° of the same scale."

That the Difficulty, which here suggests itself, of providing, in the absence even of imaginary fuel, a Supply of imprisoned Heat sufficient to fuse the substances I have mentioned and others scarcely less refractory, may be mitigated by extending the time employed in the process, or by the aid of compression and other circumstances, I am ready to admit; but, in the most favourable view of the case, the Heat wanted, (when we consider the thickness and extent of these rocks, comprising entire mountains and mountain chains,) must be prodigious; and I cannot but admire the singular taste of those geological speculators, who, enjoying the free range of the globe, have deposited their Caloric exactly in that spot in which it can be of least use to them. The inconvenience of this distribution becomes still more apparent when it is recollected that fusion is not all that is necessary; but that, when fused, these substances must be propelled in a determinate direction and with sufficient force, in many instances, to raise the bed of the sea to the height of an Alpine chain. I will not attempt to point out to you the way in which this is accomplished, but confess at once that I do not understand it.

And yet it appears certain that the surface of our planet has become cooler and cooler, from the period when organic life commenced to the tertiary epoch. If this cannot be explained by the Escape of Heat, there remains only one other mode of explaining it,—a continually diminishing Supply. The latter is the explanation offered by Mr. Lubbock. Sir John Herschel, also, has brought into view causes within the range of physical astronomy which, independently of a Loss of Internal Heat, produce a slow but certain Diminution of Temperature on the surface of our globe*. These auxiliaries, however, are insufficient.

Mr. Lyell has offered another solution of the problem, depending

* The Baobab-tree of Senegal is supposed by Adanson to have attained the age of 5150 years, and De Candolle attributes to the *Cupressa disticha* of Mexico a still greater longevity. (Lyell, vol. iii. p. 99.)

If these opinions be correct, it seems improbable that any great change either of level or climate can have taken place at these spots within the last 5000 years.

not on celestial but terrestrial causes. The chapter that contains it abounds in valuable information and ingenious reasoning; but when the author tells us that* in every country "*the land has been in some* "*parts raised, in others depressed, by which and other ceaseless changes,* "*the configuration of the earth's surface has been remodelled again and* "*again since it was the habitation of organic beings, and the bed of the* "*ocean lifted up to the height of the highest mountains,*" I cannot but wish that he had stated this as an opinion, not as a fact.

All these theories have one defect in common; they do not meet the whole of the case. We have to explain not only the *Cooling gradual* during the long interval that occurred between the formation of the carboniferous beds and the chalk, but also the *Sudden Chill* which followed, and seems to have continued from that time to this. There is yet another element to be taken into account. The coal-beds of Melville Island contain various plants, natives of the country where they are found, and which, if we may trust analogy, require for their healthy growth or for their growth at all, not only tropical heat †, but a tropical apportionment of the periods of exertion and repose. It is a botanical impossibility that such plants could have flourished in a region in which they must have been stimulated by months of continuous Light, and paralysed by months of uninterrupted Darkness. The distribution of Light, therefore, as well as of Heat, must formerly have been different from what it is at present.

To meet this further difficulty, recourse is had to physical astronomy, which gives us the *Precession of the Equinoxes, and a Shifting Axis of Rotation*: but the periodical changes of astronomers are insufficient to explain the phenomena to which I have just drawn your attention. It has therefore been suggested that a greater change may, in the course of ages, have been produced on the axis of the earth's rotation by some foreign cause, say the *Collision of a Comet.*

Such change is undoubtedly possible, but of possibilities there is no end, and we must circumscribe our researches to render them useful. Sir John Herschel gives us no encouragement, therefore, to proceed with this speculation. Mr. Conybeare also dissuades us from it, but by an argument which to me at least appears inconclusive.

His argument, founded upon the lunar theory, is this,—that the internal strata of the earth are ellipses parallel to its external outline, their centres being coincident, and their axes identical with that of the surface. The present axis of the earth must therefore have been its axis from the beginning. It may have been so, yet I should like to be told by what process the form of the internal strata of the earth had been so nicely determined. Possibly, however, I may not understand the expression "internal *Strata.*" All I believe to be ascertained is, that of corresponding sections of the interior the density is nearly the same, and if so, my inference is, not that the earth has

* Principles of Geology, vol. i. p. 113.

† Since this passage was written, doubts have been expressed whether the specimens of these plants preserved at the British Museum are sufficiently distinct to warrant the inference.

never changed its axis of rotation, but that if it has done so, the interior was then sufficiently pliant to accommodate itself to the change.

A much more formidable objection to the employment of such a cause is, that if once called in, we must take it with all its consequences. The effects produced by it will not be what we wish performed, but what its nature obliges it to perform. In explaining the phænomena of Melville Island, it might render inexplicable those of the rest of the world. If we choose to change the axis upon which the earth revolves, let us at least fix upon the best time for doing it; now what is that time? immediately after the formation of the carboniferous series? The reduction of temperature at that epoch was inconsiderable; tropical plants and animals are found in the lias, in the oolite series, in the chalk. A much more convenient time would be on the first appearance of the tertiary rocks; but however satisfactory it might be to trace to such a cause the violent changes and disturbances which appear to have taken place about that period in all other parts of the world, I am afraid our satisfaction would be greatly diminished on finding that Gosau and Maestricht * escaped unhurt.

Be the cause what it may, the effect is certain. The Temperature of the Crust of the Earth must have been higher when the Coal-measures were deposited than now, and we have reason to think it was still higher at antecedent periods. That a considerable degree of Heat still exists, either partially or generally, at no great distance from the surface, appears from thermal springs and volcanoes.

I am aware that the doctrine of *Internal Cavities* has been regarded as visionary; and in the extent to which it was carried by some of the old Cosmogenists it was so; but that comparatively near to the surface, there are, I do not say Vacuities, but large Spaces unoccupied by solid matter, is not only probable, but almost proved. It seems, indeed, to be a necessary consequence of the structure of the crust of the earth. No miner has ever got to the bottom of a vein, and a vein itself is often a half empty pipe or fissure. The correspondence of the phases of distant volcanoes, the continuous ranges of their eruptive openings, the vast extent of territory shaken simultaneously by their convulsions, are so many proofs of communication below the surface. The bulk of the ejected matter cannot be less than that of the concreted ejections which we see; for at the temperature of fusion it is greater than at a lower temperature, and for every foot of matter ejected, it is necessary to provide a substitute in the place which it occupied.

The continuous streams of lava which issued in Iceland, on one occasion, attained the length of forty or fifty miles. But the bulk of volcanic matter presented to view, does not enable us to form a correct estimate of the quantity of matter ejected; we must take further into account the combustible substances which have vanished, the gases which have escaped, the dust and ashes which, projected into the air, have fallen many miles distant from the place of explo-

* See the descriptions of these places in Geol. Trans.

sion *. Then only can we entertain a just idea of the Cavities that must have been created in the interior of the earth by the escape of a mass of matter competent to produce an Etna or a Chimboraço. Such Cavities are ill suited to support such Mountains; La Metherie therefore supposes Cavities to be at a distance, and volcanic matter to flow from these through long galleries and fissures of communication. Nor have we in volcanic countries alone decisive evidence of the existence of subterranean Cavities. No rock is exempt from Fissures: in thick beds of limestone Fissures and Caverns are exceedingly abundant; and the extent of these last is sometimes prodigious. Who has not heard of the Grotto of Antiparos? of the Caverns of Carinthia and Carniola, the content of which amounts to some hundred thousand cubic feet? of the Kingston Cave recently explored near Michelstown in Ireland?

To the frequency of Caverns and Openings, by whatever name designated, I ascribe many of the inequalities which vary the surface of the earth; such openings, I conceive, produce phenomena sometimes of Subsidence, sometimes of Elevation. I cannot entertain a doubt, that many of the tilts and contortions of strata usually ascribed to *Soulèvement*, have been occasioned solely by want of adequate support.

The Duchy of Finland exhibits an endless series of lakes filling up the hollows of a granitic surface. Let me be allowed a similar series of subterranean lakes occupying similar basins beneath the level of the Baltic, and receiving, by means of Fissures extending up to the summits of the Scandinavian chain, a continual supply of water which has no outlet; in other words, let me be allowed the use of hydrostatic pressure; and without having recourse to central heat or secular refrigeration, I think I shall be able to account, without difficulty, not by a general and uniform Rising, but by a number of unequal and partial Risings, for the phenomena observed along the shores of the Baltic.

Steam is often referred to as capable of producing the same result, nor will I deny that it might do so under favourable circumstances; but I apprehend Steam rarely does act in nature under such circumstances; for its existence depends on the access of heat, and its force on close confinement, contingencies not very likely to occur in the porous and fissured strata of the earth. Any of the various Gases, if compressed, might also become agents of elevation, but only under the same conditions as steam.

I have reserved for the last the popular theory which accounts for Elevation by the forcible *Inroad of igneous rocks into sedimentary.*

To put this theory to the test, it is natural to inquire, what igneous rocks are. My answer is, whatever geological speculators think proper to call so. The late Professor Dugald Stewart cautioned us strongly, though, alas! in vain, to avoid the language of theory. Appearances, he observes, "should always be described in terms which involve no "opinion as to their causes. These are the objects of separate ex-"amination, and will be best understood if the facts are given fairly,

* In 1783, a submarine Volcano off the coast of Iceland ejected so much pumice that the ocean was covered to a distance of 150 miles, and Ships were considerably impeded in their course.

"without any dependence on what should yet be considered as un-"known; this rule is very essential where the facts are in a certain "degree complicated."

In dealing out to rocks the appellation of *igneous,* some geologists are more liberal than others. I have not time to enumerate the various rocks which enjoy this title, still less to investigate their respective claims to retain it. I will therefore content myself with observing, that in the scantiest catalogue they are many in number, and consequently, if ejected in a state of fusion, must have been ejected from different reservoirs and cauldrons, not from a *central* cauldron.

That any rock whatever was originally igneous, is a gratuitous assumption. Lavas themselves may be, and probably are, in very many cases, Rocks not originally igneous, but Rocks which have been exposed at one time or other to the action of fire.

Granite is one of the rocks most usually considered as an *Agent in Elevation,* for what reason I am at a loss to discover. Solid Granite has no inherent principle of motion; if it move, it can only be by virtue of the impulsion it has received from some other body, not in consequence of its igneous origin or its want of stratification. The disturbances of strata that adjoin granite are not more constant, nor more striking nor more extensive than those of strata far remote from it, as for instance, the limestone shales of Derbyshire or the coal-beds of Liege. Granite veins are too small to raise mountains, and the changes or anomalies that take place at the junction of granite with other rocks, whatever else they may prove, appear to me to have no bearing on the question of *Elevation.* On the other hand, the arguments adduced against the doctrine that Granite while fluid has been forcibly injected from beneath into its present position, are to my mind conclusive; especially that which is founded on the frequent transition which takes place from Granite to the rocks that adjoin it. We find a continuous series from Granite through Gneiss and Mica slate to Clay-slates and the Fossiliferous Slates; and it is not possible to stop at any point of this progress, and to say in which direction the tendency is strongest. If the gradation were single, the difficulty would be great, but what shall we say to a repetition of such gradations? In Mr. Weaver's paper on the East of Ireland, two detailed sections are given, in one of which, more than six layers of Granite alternate with as many of Mica slate, and in the other five alternations of the same kind occur, the rocks in each instance forming bands from three to seventy fathoms in thickness.

The reliance which some authors place on Granite and other unstratified rocks, as *Agents of Elevation,* is to me very extraordinary; let one instance suffice. At Castrogiovanni in Sicily, the Pleiocene Beds attain an altitude of three thousand feet; hence it has been inferred, that *since these beds were deposited, there has been formed and introduced into the beds subjacent, a body of Granite, Sienite, Porphyry or other crystalline and unstratified Rocks three thousand feet in thickness.* This supposition is said to be necessary, but since I do not see the necessity, I will venture another supposition, viz. that Etna has not risen to the height of ten thousand feet without occasioning large

cavities in its neighbourhood, some of them submarine; that Castrogiovanni is situate over one of these; that the Pleiocene strata have closed the cavity and rendered it water-tight, except on the side of Etna; from whose lofty flanks and cloud-capped crater the caverns beneath are regularly supplied by fissures with rain-water and melted snow. Let the author grant me so much,—I ask no more. The hydrostatic paradox has tripped up the hills of the geological one, and I behold my Pleiocene beds mounted at once on a pedestal three thousand feet high, and capable of still further promotion.

If the explanation here offered meets the case of Castrogiovanni, it will equally account for the height of the tertiary beds in different parts of the Val di Noto, and for similar phenomena in every country which is or has been formerly the site of volcanic eruptions.

To the appearances on the Gulf of St. Lawrence, described by Captain Bayfield, I have already adverted.

My Predecessor directed your attention last year to the existence in the Morea of four or five distinct Ranges of ancient Sea cliffs, marked at different levels in the limestone escarpments by lithodomous perforations, lines of littoral and sea-worn caverns, and other striking proofs of former tidal action. Similar Terraces have been observed in Sicily, in Chili, in the Gulf of St. Lawrence and various other places. At Uddevalla in Sweden, are ancient Beaches with shells of living species, two hundred feet above the level of the Baltic, a height strikingly disproportionate to the very moderate Rise ascertained to have taken place in other parts of the Scandinavian coast: many examples of similar phenomena have been found in Great Britain. It would be rash to offer a solution of these phenomena in the gross. Every individual case deserves separate examination. All I undertake at present is to put a new key into the hands of the decipherer.

It was my intention on commencing this address to have discussed at some length the theory of M. Elie de Beaumont, but there is not time now to do it justice. He belongs to that class of authors whose opinions, right or wrong, always instruct me. There is no part of his theory which does not evince thought and diligence, a habit of correct observation and an enlarged mind. In some respects I differ from him, and it will not be difficult to infer from what I have already said, wherein the difference consists. Should these observations engage his notice, I would beg him to consider whether the disturbances in the Alps and elsewhere have not been generalized rather more than they will bear, whether the tilts and upliftings may not have taken place bit by bit at various epochs, and whether, if the *secular Refrigeration of the Globe* cannot be established, and *Central Heat* be an *Ignis fatuus*, his attention may not be usefully directed to more partial but better authenticated sources of disturbance and elevation.

Allow me, in conclusion, to say a few words upon a subject in connexion with which my name has of late been brought forward much more prominently than I could have desired;—I mean *Diluvial Action.*

Some fourteen years ago I advanced an opinion, founded altogether upon physical and geological considerations, that the entire earth had, at an unknown period, (as far as that word implies any

determinate portion of time,) been covered by one general but temporary Deluge. The opinion was not hastily formed. My reasoning rested on the facts which had then come before me. My acquaintance with physical and geological nature is now extended; and that more extended acquaintance would be entirely wasted upon me, if the opinions which it will no longer allow me to retain, it did not also induce me to rectify. New data have flowed in, and with the frankness of one of my predecessors, I also now read my recantation.

The varied and accurate researches which have been instituted of late years throughout and far beyond the limits of Europe, all tend to this conclusion, that the geological schools of Paris, Freyberg and London have been accustomed to rate too low the various forces which are still modifying, and always have modified, the external form of the earth. What the value of those forces may be in each case, or what their relative value, will continue for many years a subject of discussion; but that their aggregate effect greatly surpasses all our early estimates, is I believe incontestably established. To Mr. Lyell is eminently due the merit of having awakened us to a sense of our error in this respect. The vast mass of evidence which he has brought together, in illustration of what may be called *Diurnal Geology*, convinces me that if, five thousand years ago, a Deluge did sweep over the entire globe, its traces can no longer be distinguished from more modern and local disturbances. The first sight of those comparatively recent assemblages of strata, which he designates the *Eocene*, *Meiocene* and *Pleiocene* Formations, (unknown but a few years ago, though diffused as extensively as many which were then honoured with the title of universal,) shows the extreme difficulty of distinguishing their detritus from what we have been accustomed to esteem Diluvium. The Fossil Contents of these formations strongly confirm this argument. M. Deshayes has shown that they belong to a series unbroken by any great intervals, and that, if they be divided from the secondary strata, the chasm can have no relation to any such event as is called The Flood.

Further, the elephants and other animals once supposed to be exclusively *Diluvial*, are now admitted to be referrible to two or three distinct epochs; and it is highly probable that the blocks of the Jura Mountains, of the North of Germany, of the North of Italy, of Cumberland, Westmorland, &c., are not the waifs and strays of one, but of several successive Inundations.

It is, Gentlemen, a well-known rule of such institutions as ours, that the "Authors alone are responsible for the facts and opinions contained in their respective productions." Under that feeling have I spoken on the present occasion, and having freely set before you what has occurred to me on some points of general interest to our science at this time, I think it my duty, in concluding this address, to disclaim and deprecate any attempt to connect what I have here expressed with the general sentiments of the Geological Society. The opinions I have uttered are my own, and I should be sorry that more importance should be attached to them than they intrinsically deserve, from the accident of their having been delivered from this

Chair. Had not the whole responsibility fallen on myself, I should have hesitated, or perhaps altogether forborne to bring before you Opinions, several of which I know are little in accordance with those of some of the most distinguished members of our association.

LINNÆAN SOCIETY.

June 3.—A Paper was read entitled, "On certain Deviations from the ordinary Structure in *Telopea speciocissima*. By Mr. David Don, Libr. L.S."

In a large proportion of the *Proteaceæ*, Mr. Don observes, including *Telopea*, the filaments are firmly united along their whole length to the inner surface of the foliola of the perianthium, which apparently bearing the anthers in their concave apices, look as if they constituted but a single series of organs, performing the double functions of stamina and perianthium. In a spike, however, of *Telopea speciocissima*, which blossomed at Mr. Knight's Nursery, in May 1833, Mr. Don found a number of flowers in which some of the filaments were entirely free, and with the anthers more developed than are met with in the ordinary state; a circumstance which may have arisen from the oblique direction given to the stamen in a very early stage of the flower, (at which, in ordinary cases, the cohesion most probably takes place,) and thus, in this instance, preventing cohesion from taking place between it and the opposite leaf of the perianthium. The pistillum of this plant presents a beautiful example of adaptation. As the leaves of the perianthium approximate closely at the base,—being there so narrow as to have prevented the development of the ovarium, had it been sessile,—we find it elevated on a stalk, so as to place it in the widest part of the tube formed by the leaves of the perianthium, and, at the same time, to raise the stigma on a level with the anthers, the ovarium appearing as if situated in the middle of the style. After some further remarks on the structure of the pistillum, the author concludes by showing that the figure of *Telopea speciocissima* in 'Exotic Botany' is very faulty, the position of the flowers with respect to the axis of the spike being entirely reversed.

An addendum to Mr. Thompson's paper was also read, containing a notice of two specimens of the Noddy, *Sterna stolida*, Linn., which were shot, a few years since, at sea, between the Tusker lighthouse off the coast of Wexford, and the Bay of Dublin. Both are in mature plumage; one of them is now preserved in the collection of Thomas W. Warren, Esq., of Dublin, and the other in that of W. Massey, Esq., of the Pigeon House, in the same city, who originally received both specimens from the captain of the vessel on board which they had been killed.

June 17.—A Paper was read "On the Female Flower and Fruit of *Rafflesia*, with observations on its affinities, and on the structure of *Hydnora*." By Robert Brown, Esq., V.P.L.S.

The author's principal object in this paper is to complete his account of *Rafflesia Arnoldi*, the male flower of which he described in a former communication, published in the 13th volume of the Society's

Transactions; and, in connexion with the question of its place in a natural arrangement, he introduces a more detailed description and figures of *Hydnora africana*, than have hitherto been given. The drawings of Rafflesia which accompany the paper are by Francis Bauer, Esq., and those of Hydnora by the late Mr. Ferdinand Bauer.

From a comparison of Rafflesia with Hydnora and Cytinus, he is confirmed in the opinion expressed in his former paper, but founded on less satisfactory evidence, that these three genera, (to which Brugmansia of Blume is now to be added,) notwithstanding several remarkable peculiarities in each, may all be referred to the same natural family; and this family, named by him *Rafflesiaceæ*, he continues to regard as being most nearly allied to Asarinæ.

He does not, however, admit an arrangement lately proposed by M. Endlicher, and adopted by Mr. Lindley, by whom these genera are included in the same natural class with *Balanophoreæ* of Richard; an approximation founded on their agreement in the structure of Embryo, and on the assumed absence of spiral vessels. On this subject he remarks, that in having a homogeneous or acotyledonous Embryo, they essentially accord, not only with many other plants, parasitical on roots, which it has never been proposed to unite with them, as Orobanche, &c., but also with Orchideæ, their association with which would be still more paradoxical. And with respect to the supposed peculiarity in their vascular structure, he states that he has found spiral vessels not only in Rafflesia, (in which he had formerly denied their existence,) and in Hydnora and Cytinus, but likewise in all the Balanophoreæ examined by him, particularly Cynomorium and Helosis, as Dr. von Martius had long since done in Langsdorfia, and Professor Meyer very recently in Hydnora.

In his observations on the ovulum of Rafflesia, he gives a view of its early stages of development, and which he extends to Phænogamous plants generally, in some respects different from that taken by M. Mirbel, who considers the nucleus of the ovulum, in its earliest state, as inclosed in its coats, which gradually open until they have attained their maximum of expansion, when they again contract around the nucleus, and, at the same time, by elongating, completely inclose it. Mr. Brown, on the other hand, regards the earliest stage of the nucleus as merely a contraction taking place in the apex of a preexisting papilla, whose surface, as well as substance, is originally uniform, and that its coats are of subsequent formation, each coat consisting, at first, merely of an annular thickening at the base of the nucleus, which, by gradual elongation, it entirely covers before impregnation takes place.

But this mode of development of the ovulum, he remarks, though very general, is not without exception; for in many, perhaps in all, Asclepiadeæ and Apocineæ, the ovulum continues a uniform cellular tissue, exhibiting no distinction of parts until after the application of the pollen tube to a definite part of its surface, when an internal separation or included nucleus first becomes visible.

ZOOLOGICAL SOCIETY.

February 25.—A letter was read, addressed to the Secretary by M. W. Bojer, Corr. Memb. Z.S., and dated Mauritius, Nov. 15, 1833. It referred principally to the animal from Madagascar, which was transmitted in the spring of last year to the Society by the late Mr. Telfair, and which was brought by Mr. Bennett on April 9, 1833, under the notice of the Society as the type of a new genus, for which he proposed the name of *Cryptoprocta*, on account of its possessing an anal pouch, and being thereby distinguishable from *Paradoxurus*, F. Cuv. One of the habits of the *Cryptoprocta ferox* indicated, during the life of the animal, the existence of this pouch: when violently enraged, and it was apt to become exceedingly ferocious on the sight of a morsel of flesh, "it frequently gratified the persons present with, not an odoriferous, but a most disagreeable smell, very like that of *Mephitis*." Other particulars were contained in the letter, which are given in the 'Proceedings.'

The reading was commenced of a Paper, entitled "Descriptions of New Species of *Calyptræidæ*: by W. J. Broderip, Esq."; and the *Shells* described in it, chiefly obtained from the collection of Mr. Cuming, were exhibited.

Mr. Owen read a Paper "On the Anatomy of the *Calyptræidæ*." After referring to the account given by Cuvier of the anatomy of *Crepidula*, to that by M. Deshayes of *Calyptræa*, and to M. Lesson's of *Crepipatella*, as elucidating the general plan of organization in this family, he proceeds to describe the structure of *Calypeopsis*. An abstract of the paper is given in the 'Proceedings.'

Numerous specimens were exhibited of *Birds* collected in North America, principally in the United States, by George Folliott, Esq., and presented by him to the Society. At the request of the Chairman, Mr. Gould brought them severally under the notice of the Meeting. His principal object being to illustrate, so far as these birds were concerned, the geographical distribution of allied or identical species, he directed his observations chiefly to the determination of those North American Birds which seemed to him to be referrible to European species, and of those which, having been generally considered as identical with European, appeared, on direct comparison, to present differences in form and colouring.

The *common Turnstone* of Europe, *Strepsilas collaris*, Temm., appears to be not only identical with the *Turnstone* of North America, but to be spread, without any tangible variation, over almost every portion of the globe. The *Sanderling*, *Calidris arenaria*, Temm., and the *Knot*, *Tringa Canutus*, Linn., are also identical in both continents; as is the *great white Heron* or *Egret*, *Ardea Egretta*, Temm. The *common Tern* or *Sea-Swallow* of England, *Sterna Hirundo*, Linn., occurs equally in North America. The *common Crow*, *Corvus Corone*, Linn., is also identical in both continents.

With respect to the *Whimbrel*, *Numenius phæopus*, Temm., and the *little Sandpiper*, *Tringa Temminckii*, Mr. Gould stated himself to be unable to determine as to their identity without the compari-

son of more specimens from America than he had yet been able to obtain for the purpose of examination.

The *Cross-bill* of North America Mr. Gould showed to be very distinct from that of Europe, the *Loxia curvirostra*, Linn.; it is one third less in all its proportions, and is somewhat less brilliant in colouring. The *Ring Dottrel* of North America is also specifically distinct from that of Europe, the *Charadrius Hiaticula*, Linn.; independently of differences in admeasurement, its semipalmated foot will always serve to distinguish it.

In addition to the *Birds* that have been already mentioned, Mr. Folliott's collection contained a series of the *Sylviadæ* of the United States, several *Fly-catchers*, the *Orphea rufa*, &c., &c.

Mr. Gray exhibited specimens of the shelly covering of a *Radiated* animal, allied to the *Echinidæ* and the *Asteriidæ*, which he regarded as the type of a new genus, and for which he proposed the name of

GANYMEDA.

Corpus hemisphæricum, depressum; depressione dorsi centrali quadrangulari.

Os inferum, centrale.

Anus nullus.

Ambulacra nulla.

"The body is hemispherical, depressed, thin, chalky and hollow.

"The back is rounded, rather depressed, flattened behind, with a rather sunk quadrangular central space.

"The sides are covered with sunken angular cavities with a small round ring, having an oblong transverse subcentral hole in their base.

"The under side is small, rather concave, with five slight sloping elevations from the angles of the mouth to the angles of the rather pentagonal margin. The edge is simple.

"The mouth is central. The vent none.

"The cavity is simple.

"The *parietes* are thin and minutely dotted, and the centre of the dorsal disc is pellucid.

"This genus is very nearly allied to the fossil described by Dr. Goldfuss in his beautiful work on Petrifactions, under the name of *Glenotremites paradoxus* (tab. 49. f. 9. and t. 51. f. 1.), with which it agrees in external appearance and form, in the possession of a sunken space on its upper surface, and in having only a single inferior pentagonal mouth. It differs from *Glenotremites* by being unfurnished with *ambulacra* running from the angle of the mouth to the margin, by being unprovided with conical cavities between those near the mouth, and by having in the flattened disc on the back a central quadrangular impression instead of the pentagonal star of that genus.

"Dr. Goldfuss describes the glenoid cavities on the surface as giving attachment to spines similar to those of the *Turban Echini*, (*Cidaris*, Lam.), and states that the under surface is covered with very small tubercles to which he believes spines were attached. The

cavities on the surface of *Ganymeda* and the pits in them have very much the form of those figured by Dr. Goldfuss in his fossil, but I cannot regard them as being fitted for the attachment of spines: they have much more resemblance to the mouths of cells. So great, indeed, is this resemblance, that I entertained doubts whether the whole mass might not be a congeries of cells like the *Lunulites*, rather than the case of a single body, until I considered that it was impossible, from its form, that it could increase in size with the growth of the animal, and that its exceeding regularity proved that it must be the formation of a single creature.

"I am induced to consider these two genera, though differing in the above-stated particulars, as forming a family or order between the *Echinidæ* and the *Asteriidæ*; allied to the latter in having only a single opening to the digestive canal, and agreeing with the former in form and consistence, but differing from it in not being composed of many plates.

"I only know two specimens of this genus, which I believe were found on the coast of Kent, as I discovered them mixed with a quantity of *Discopora Patina* which I collected several years ago from *fuci* and shells on that coast. The specimens are $\frac{1}{8}$ of an inch in diameter.

"I propose to call the species *Ganymeda pulchella*."

FRIDAY-EVENING PROCEEDINGS AT THE ROYAL INSTITUTION OF GREAT BRITAIN.

April 11.—Mr. Faraday on the definite action of Electricity.—This was an experimental exposition of a part of his seventh series of *Researches, &c.*, which has been reported at p. 293 of our last volume.

April 18.—No meeting, in consequence of the burial of Mr. Fuller. This gentleman had, in the foundation of funds and the endowment of Professorships, appropriated no less a sum than £10,000 to the service of science and the welfare of the Royal Institution.

April 25.—Mr. Davidson on the pyramids of Egypt, illustrated from his personal examination, and by models from measurements and drawings made on the spot.

May 2.—Dr. Lardner on Babbage's calculating machinery, illustrated by numerous drawings and models.

May 9.—Dr. Dalton. Review of his scientific life, and especially of the development of the atomic theory.

May 16.—Mr. Cowper. Illustrations of recent improvements in Calico-printing.

May 23.—Dr. Williams on a new law of combustion.—This was the matter of a Paper read at the Royal Society, which has been reported p. 440 of our last volume.

May 30.—Dr. Lardner on Babbage's mechanical notation.

June 6.—Dr. Grant on the development of the vertebral column in the animal kingdom.

June 13.—Mr. Faraday on new applications of the distilled products of caoutchouc or Indian rubber.—*Conclusion of the Season.*

XI. *Reviews, and Notices respecting New Books.*

An Inaugural Lecture on the Study of Botany, read in the Library of the Botanic Garden of Oxford. By Charles Daubeny, M.D., F.R.S., Professor of Chemistry and Botany in the University of Oxford. 1834.

THE two Universities of this country have exhibited in succession what would elsewhere be considered an anomaly or abuse,—a professorial chair continuing to be held by an individual who gave no lectures. That such a circumstance could be allowed, can only be accounted for by conceiving a general indifference to exist in regard to subjects on which these Professors had been appointed to lecture. That botany should be the branch of science and education so neglected must seem strange to every one acquainted with what botany really and truly is, while, in respect to others, the neglect is as easily accounted for as it is merited. That a different fate in both Universities now awaits it, we have no hesitation in avowing our belief. If asked on what grounds we found this expectation, we reply, on the different view which is taken of botany and its objects, and the different method pursued in teaching it. The effects of this change have begun to show themselves at Cambridge; and now, a voice having proceeded from the botanical wilderness at Oxford, every one interested in the progress of the science must feel a desire to know what sounds it will utter,—whether the uninviting language of its predecessors, or the attractive strains of sound reasoning and the inductive philosophy. Fortunately, the new Professor has furnished us with a document,—a confession of faith,—from which we may learn his opinions and views. This introductory lecture gives, as all proper introductory lectures should do, an outline of the plan to be pursued in teaching the science, the details of which will be supplied in the subsequent ones, all of these being a continued comment on the first. The faculties of mind necessary to form a botanist are stated at the commencement, from which it will be seen that Dr. Daubeny, very correctly, considers him a botanist whose mind is imbued with the great principles, by means of which plants can be collected into natural groups, and who strives to discover the general relation in which these groups stand towards each other,—in short, who labours to construct and perfect a method, " where the very place which a plant occupies in it shall, in a manner, announce its most prominent characters, the qualities it may possess, and its affinities with others." This declaration of the Professor of his intention to explain the principles of the natural arrangement of plants has given us sincere gratification, but which suffers some diminution from perceiving that some of the old leaven lurks in his mind, and that he casts a lingering look behind at the fading glories of the artificial system, the nature of which, and its use as a dictionary, he means *first* to explain. Now, we are of opinion that the *juste milieu* plan will not be found to answer in botany any more than it does in politics. We do not know what is the

mental calibre of those whom the Professor expects habitually to address; but, if it be greater than that of children or mere boys, we would proceed directly to the point which it is avowed is the ultimate one—to which, indeed, all have recourse at last, if they continue the practical investigation of plants.

We hold the pertinacious attachment to the artificial system to be the cause of the low degree of estimation in which botanical science is, therefore deservedly, held in this country. If we are right in this opinion, it is certainly greatly to be regretted, that that which was formerly dignified with the name of Botany should yet linger in our schools, or that any should be found to teach it, since there has arisen in its stead a science which is beautiful, philosophical, and capable of the most varied and useful applications,—capable of being applied to medicine, horticulture, to entomology, chemistry, and, above all, to climatology, and, consequently, to geology. The method of Jussieu does for the Vegetable Kingdom, that which the method of Macleay does for the Animal, viz. by putting us in possession of a single fact, or a few facts, it confers upon us the power of inferring many more, relating not only to the structure of the plants, but to the juices circulating in the vessels, and the products elaborated therefrom. If something more than the *name* of a plant be comprehended in botanical arrangements, let us imitate the example of Linnæus, who, conscious of the inadequacy of his artificial system to serve the cause of genuine botany, wisely abandoned it, and devoted himself to devising a natural method, and called upon all botanists to assist in accomplishing so desirable an object. Let us no longer cling to this system, which has been expelled from almost every other country of Europe,—but rather let us

> Cast it, like an idle weed, away,

which cannot be suffered longer to deform the fair garden of philosophic truth. The lecture before us contains abundant proofs of the soundness of the principles of the natural method, and of the interesting confirmation of those which the respective parts of plants afford. To it we refer, and advise all who may feel any concern in the progress of botany to procure it, which will be at once productive of pleasure to them, and of benefit to the Botanic Garden at Oxford, as the profits arising from the sale of it are to be devoted to the improvement of that, one of the earliest in this country. To aid in this object, the Professor and many Members of the University have already liberally contributed, and we trust that their praiseworthy example will be extensively followed.

Oxford contains many amiable and enlightened men, well disposed to see instruction in the natural sciences form a part of the regular academical course of education. By the strenuous and able exertions of the new Professor, these cannot fail to become more numerous. We therefore wish him the success he deserves in the prosecution of his labours, and by which the cause of botany will be greatly promoted, and its sphere of usefulness extended. ***

XII. *Intelligence and Miscellaneous Articles.*

RECENT DISCOVERY OF BONES OF THE IGUANODON.

An interesting specimen of this fossil reptile has been lately discovered in the Shanklin sand formation, in the immediate neighbourhood of Maidstone in Kent. The quarry in which these remains occur consists of many strata, regularly alternating, of compact limestone, and of sand more or less loose. Each stratum is of the thickness of from 8 inches to 12 or 14; and the alternation of the two species of beds is remarkably regular and equal.

It unfortunately happened that the blast by which these remains were brought to light was inserted into what proved to be the centre of this very deranged specimen, so that the whole mass containing it was shattered into many pieces. By the great care bestowed upon them, however, by the very intelligent proprietor of the quarry, Mr. W. H. Bensted, nearly all the detached pieces have been collected, and the various bones carefully cleared from the rock which forms their matrix. These remains, together with the other fossil bodies contained in the same strata, cannot fail to prove of deep interest in a geological point of view. That both the sand and the limestone are *marine* formations there can be no doubt, for though wood and vegetable substances are not uncommon in these beds, yet the limestone abounds in ammonites, sharks' teeth, and other sea productions, while a small sea shell was also found fixed upon one of the bones of the iguanodon. These bones (which have been recognised on the spot as belonging to this species, by Mr. Mantell,) consist of two femora, a tibia, fibula, 15 vertebræ, 2 clavicles, 2 *claws*, 2 teeth, some ribs, &c. One of the femora is 32 inches long, and its position with respect to the mineral beds inclosing it is worthy of particular attention, as throwing much light upon the manner of the deposition of these beds. It stands nearly in a *vertical* position as regards the strata, which are nearly horizontal; and it projects from the solid limestone bed, which embraces its lower extremity, and passes nearly through the superincumbent bed of sandstone. Here is distinct proof that these two beds, now so different in their consistency, were, in the one case, *loose sand*, and in the other, *tenacious mud*, at the period when this *shattered* and *decomposing body* sunk to the bottom of the sea, and became covered up by an abundant deposition. Nor can we doubt that this deposition took place *most rapidly*, as this long bone must then have been in a loose and moveable condition, (probably only attached to the other bones by strong integuments,) and could not long have retained an upright position. But if this inference be just with respect to the nature of the deposition of those *two* beds, we cannot avoid coming to the very same conclusion *with regard to all the other beds of the series*, of a nature precisely similar, and extending to a depth of many feet, in the quarry.

We have thus, in this instance, proof of *marine* origin, and of a rapidity of deposition greater than we can well calculate upon, in exist-

ing marine action. This proof leads to the same conclusion (though on a smaller scale), as the *vertical* stems of large trees in the coal and other secondary strata, intersecting many distinct beds: and this *more than natural* rapidity of deposition, which is not yet acknowledged by geologists in general, cannot fail soon to attract attention, and to exercise a powerful influence on many opinions at present generally received, which are entirely opposed to it.

G. F.

June 10, 1834.

MURIATIC ACID IN FLUOR SPARS.

M. Kersten states that in examining the results of the decomposition of certain minerals in which he found that chlorine and fluorine frequently replaced each other, he suspected that fluor spars might probably contain chlorine or muriatic acid, and this idea was confirmed by finding small quantities of it in several varieties of the blue fluor spar of Marienberg, and in some of those of Freyberg.—*Ann. de Chim. et de Phys.*, tom. liii. p. 324.

POLYSPHERITE, OR BROWN PHOSPHATE OF LEAD.

This mineral was found in 1830 near Freyberg: it consists of globules and isolated drops placed upon each other, in the interior of which a great quantity of concentric radii are visible. Its lustre is greasy: its colour passes from clove brown to Isabella yellow. Its fracture is radiated: its hardness is equal to that of calcareous spar, and its density, according to M. Breithaupt, is 6·092.

M. Kersten found it to consist of

Oxide of lead	72·17	or	Chloride of lead	10·838
Lime	6·47	—	Fluoride of calcium ..	1·094
Muriatic acid	2·00	—	Subphosphate of lead .	77·015
Phosphoric and muriatic acids and loss	19·36	—	Subphosphate of lime .	11·053
	100			100

Ibid.

NEW RADICAL ANALOGOUS TO CYANOGEN.

M. Liebig states that he has found a new radical composed of 3 atoms of carbon and 5 of azote; it is a pulverulent body, insoluble in water, and decomposes at a red heat, into azote and pure cyanogen, in the proportion of 1 : 3. It combines with potassium, and furnishes, by combining with acids and alkalies, a series of new combinations. One of these bodies treated with nitric acid gives ammonia, and a new acid, which is soluble in water, and crystallizes on cooling in plates of a brilliant metallic appearance. This acid is perfectly similar in composition to cyanuric acid, but the weight of its atom is double; its formula is $C^6\ N^6\ H^6\ O^6$. By distillation hydrated cyanic acid, &c., are obtained.—*Ann. de Chim. et de Phys.*, tom. liv. p. 252.

ON SUBOXIDE OF LEAD AND PROTOXIDE OF TIN. BY M. BOUSSINGAULT.

By subjecting oxalate of lead to dry distillation, M. Dulong obtained a black powdery residue, which he considered as lead of a low degree of oxidation. M. Berzelius is of opinion that this new oxide is produced when metallic lead is exposed to the atmosphere. The existence of this suboxide is not, however, admitted by all chemists, and its composition has not been determined.

To determine the question M. Boussingault prepared suboxide of lead, by decomposing the oxalate in a small glass retort. To obtain it pure the body of the retort must be kept at a low red heat; at a higher temperature some globules of lead are produced, and the glass is attacked.

When the disengagement of gas has ceased, the retort must be allowed to cool without access of air. This is easily managed by adapting a long tube to the retort and plunging it into mercury.

The suboxide of lead is of a very dark gray colour, almost black. When heated below the fusing point of lead, it is converted into oxide. Sulphuric, muriatic, and acetic acids attack it, especially when heated; oxide of lead is formed, which combines with the acids, and metallic lead separates.

When mixed with water and exposed to the air, the suboxide is quickly converted into carbonate of lead; under water, and without the contact of the air, it suffers no change. Mercury rubbed with the suboxide under water does not dissolve lead. This experiment seems to prove that suboxide is not, as has been supposed, a mere mixture of lead and oxide.

In order to determine the composition of this suboxide it was converted into oxide, and the absorption of oxygen noted: this was effected by heating it in a cupel to nearly a red heat. In two experiments 100 parts of suboxide became 103·6 of oxide, containing 7·2 of oxygen; and as the oxygen which combined with the suboxide during calcination amounted to 3·6, it is evident that in the suboxide the lead is combined with precisely half as much oxygen as exists in the oxide. The suboxide may therefore be considered as constituted of 1 atom of oxygen + 2 atoms of lead.

Oxalate of tin prepared by pouring oxalic acid into protacetate of tin, yielded by distillation water, oxide of carbon, carbonic acid, and empyreumatic oil. The residue was protoxide of tin of a bright brown colour.—*Ibid.*, p. 264.

SCIENTIFIC BOOKS.

Researches in Theoretical Geology. By H. T. De la Beche, F.R.S., V.P.G.S.

Outline of the Geology of the Neighbourhood of Cheltenham. By R. I. Murchison, F.R.S., V.P.G.S.

Report of the British Association for the Advancement of Science at Cambridge in 1833.

Origines Biblicæ; or Researches in Primæval History. By Charles T. Beke.

Meteorological Observations made by Mr. THOMPSON *at the Garden of the Horticultural Society at Chiswick, near London; by Mr.* R. HOCKING *at Penzance, and Mr.* VEALL *at Boston.*

Days of Month. 1834.	Barometer.					Thermometer.					Wind.			Rain.		
	London.		Penzance.		Boston $8\frac{1}{2}$ A.M.	London.		Penzance.		Bost. $8\frac{1}{2}$ A.M.	Lond.	Penz.	Bost.	Lond.	Penz.	Bost.
	Max.	Min.	Max.	Min.		Max.	Min.	Max.	Min.							
May 1	29·785	29·665			29·13	63	38			54	sw.		calm	0·02		0·07
2	29·893	29·875			29·31	67	48			58	s.		w.	...		...
3	30·088	29·966			29·33	75	40			58·5	sw.		calm	...		...
4	29·973	29·931			29·36	77	58			61	s.		w.	·16		...
5	30·111	29·932			29·25	70	42			62	s.		calm	·44		·12
6	30·366	30·289			29·58	71	47			60	s.		calm	...		·17
7	30·446	30·403			29·64	75	46			64·5	s.		calm	...		...
● 8	30·322	30·091			29·55	80	48			61	sw.		calm	...		...
9	29·860	29·778			29·13	75	45			64	sw.		calm	...		...
10	29·952	29·913			29·36	73	39			56	sw.		w.	...		...
11	29·837	29·729			29·27	75	52			59	s.		calm	·04		...
12	29·746	29·715			29·01	67	51			60·5	s.		calm	·40		...
13	29·636	29·595			28·95	66	47			58	s.		calm	·03		·20
14	29·788	29·764			29·11	69	52			56	s.		calm	·02		·14
15	29·956	29·851			29·28	75	45			60	s.		calm	...		·03
☽ 16	29·939	29·709			29·46	71	48			62	E.		calm	...		...
17	29·498	29·435			29·46	61	41			61	w.		calm	·08		...
18	29·615	29·451			28·93	64	38			50	sw.		calm	...		·08
19	30·078	29·775			29·33	67	36			56	sw.		w.	...		...
20	30·426	30·303			29·77	73	39			57·5	s.		w.	...		...
21	30·501	30·461			30·	70	44			62	E.		calm	...		...
○ 22	30·435	30·358			30·03	67	47			58	NE.		E.	...		...
23	30·318	30·286			29·85	72	45			61	E.		E.	...		...
24	30·463	30·365			29·94	74	47			60	E.		E.	...		...
25	30·460	30·370			30·03	64	39			57	NE.		E.	...		...
26	30·347	30·321			29·93	66	41			56	E.		E.	..		...
27	30·293	30·247			29·81	69	44			60	NE.		NE.	...		...
28	30·212	30·181			29·75	66	35			57·5	E.		NE.	...		...
29	30·146	30·057			29·67	76	48			56	NE.		calm	...		...
☾ 30	30·182	30·107			29·66	73	44			53	E.		E.	...		...
31	30·275	30·230			29·78	72	40			58	NE.		calm	...		...
	30·501	29·435			29·50	80	35			58·6				1·19		0·81

Remarks.

London.—May 1. Rain. 2. Fine. 3. Very fine: clear. 4. Sultry: rain at night. 5. Heavy rain. 6, 7. Very fine. 8. Foggy: fine. 9. Heavy dew: overcast. 10. Fine: 11. Fine: lightning at night. 12. Slight rain: cloudy and fine: rain. 13. Heavy showers. 14—16. Fine. 17. Rain. 18. Cloudy: clear and cold at night. 19—21. Very fine. 22—31. Fine: but very dry with easterly winds.—The dry state of the atmosphere in the latter part of the month was unfavourable to most kinds of vegetation. No rain having fallen after the 17th, the supply of moisture in the soil became much exhausted, especially as only a moderate quantity fell in the previous part of the month; and in the two preceding, the amount was not the usual average for one. It may be remarked, that the prevalence of S.W. winds, and the concomitant superabundance of rain in December and January last, appears to have been since balanced by a reaction of as long duration from the opposite quarter, and with a deficiency of rain as much below the average quantity, as this, in the former case, was exceeded.

Boston.—May 1. Cloudy: rain early A.M. 2. Fine. 3, 4. Cloudy. 5. Rain. 6, 7. Fine. 8, 9. Cloudy. 10, 11. Fine. 12. Cloudy. 13. Rain. 14. Cloudy. 15. Cloudy: rain early A.M. 16. Fine. 17. Fine: rain P.M. 18. Cloudy. 19—24. Fine. 25. Cloudy. 26—31. Fine.

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[THIRD SERIES.]

AUGUST 1834.

XIII. *Experiments and Observations on the Action of Water and Air on Lead. By* Captain PHILIP YORKE.*

1. ALTHOUGH the action of water on lead has at different times been noticed by chemical and other writers, yet I venture to think that the experiments I have to offer comprehend some new facts, and some which may assist us in correcting views deduced from the results of previous experimenters.

2. My experiments originated in the examination of some spring water which had flowed through somewhat more than 100 yards of leaden pipe. When this water, fresh drawn from the cistern, was tested by a solution of sulphuretted hydrogen gas, it gave a clear brown tint. The quantity of oxide of lead estimated by the sulphuret obtained from $14\frac{1}{2}$ measured ounces amounted to $\frac{1}{62000}$th. When it had stood two or three days in an open vessel, it no longer gave the brown tint when tested, but some white particles suspended in the liquid were blackened.

3. This circumstance called my attention to some results obtained by Guyton de Morveau, quoted by Berzelius†, viz. "that oxide of lead is soluble in pure water, but insoluble in water which contains the least trace of salt. If distilled water is poured into a vessel of lead, and is left for some time, the lead is attacked and the water acquires the property of exer-

* Communicated by the Author.

† *Traité de Chimie,* tom. iii. p. 178.

cising a feeble alkaline action on reddened litmus paper, is rendered brown by sulphuretted hydrogen gas, and is made turbid by sulphuric acid."

4. As I had not at this time seen these facts stated in any other chemical work, I made the following experiments to ascertain their exactness. I put some distilled water into a glass, loosely covered so as to keep out dust, and filled a phial fitted with a ground-glass stopper with distilled water. I then arranged similar vessels with spring water, and into each I put a slip of clean and fresh-cut lead. The spring water used in this experiment I had previously ascertained to contain in the gallon (of 10 pounds) 1·21 grain of the chlorides of sodium and calcium, and 6·4 grains of carbonate of lime held in solution by excess of carbonic acid ($\frac{1}{9377}$th).

5. The spring water used in the above experiments with lead was tested by sulphuretted hydrogen after the lapse of three days, three weeks, and a year respectively, but never gave any indication of holding lead in solution. The lead acquired its usual dull surface at last, and became invested near the surface of the water with oxide of a reddish and brownish colour, much resembling in colour, but not in thickness, that incrusting the leaden bullets described by Mr. Faraday in the Journal of Science, vol. xvi. p. 163. The lead in the distilled water and open vessel (*a*) in five days was covered near the surface of the water by fine white flaky crystals radiating from the lead. The water when tested by sulphuretted hydrogen gave a brown tint. The same kind of action took place in the stopped phial (*b*), but much slighter. In the open vessel, at the end of three weeks, a quantity of the white crystalline substance had accumulated at the bottom of the glass, and a zone of pearly crystalline flakes of the same substance had formed on the surface of the water, adhering slightly to the glass and to the lead. A portion of this substance was collected and dried at 212°. When treated with dilute acids it dissolved: I did not perceive any effervescence. When heated in a glass tube it gave off a portion of water and became yellow.

6. The stopped phial, when examined after standing a year, presented the following appearances. The bottom of the phial was covered with about $\frac{1}{4}$ of an inch in depth of the white crystalline substance before mentioned. The slip of lead itself was covered for about $\frac{2}{3}$rds of its length from the lower end with brilliant laminar crystals (*b*) projecting from the lead about $\frac{1}{30}$th of an inch. When brushed off the lead and examined by reflected light, their colour was greenish grey, and

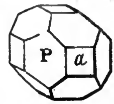
much resembled some brilliant varieties of mica: under the microscope by transmitted light they were yellowish.

7. Such are the general appearances presented by the action of water on lead. To ascertain whether any effect was produced when access of air was entirely cut off, I filled a little retort with distilled water, and boiled it for some time; I then introduced into it some fresh-cut clear slips of lead. The beak of the retort, which was quite full of water, was then immersed in a basin of mercury, the surface of which was covered with water. When this arrangement was examined after standing three weeks, not the smallest bubble of gas was visible. The lead was still bright, though a little whitish in places: the water struck a very pale brown when tested by sulphuretted hydrogen. The retort was then left open and half-filled for a night; a quantity of the white substance formed, and the water gave a deep brown when tested. It is evident from this experiment that the lead does not derive oxygen from the water, but from the air contained in it, though, like iron in similar circumstances, it is a delicate test for oxygen dissolved in water *.

8. To obtain the products of the action of air and water on lead in larger quantities, I filled a quart bottle about $\frac{2}{3}$rds of its capacity with distilled water, which I agitated briskly with the air, and introduced a parcel of clean cuttings of sheet lead: white clouds appeared in a few minutes; and the brilliant grey crystals (*b*) began to be visible on the surface of the lead after standing four days. After standing a month, the surface of the water was covered by a slightly coherent stratum of the white crystalline substance, and there was also a deposit of the same matter: the lead was covered by the brilliant grey crystals. A quantity of these last were brushed off the lead: they dissolved quietly in acetic acid. A portion introduced into a bit of glass tube, closed at one end and previously counterpoised, weighed $\frac{91}{100}$ grain. When this was heated red by a spirit-lamp, it decrepitated a little, and a very small portion of water was condensed in the cool part of the tube: when this was driven off, the loss was only $\frac{1}{100}$ of a grain. The substance had become yellow, but the structure was not visibly altered. In another trial no water was given off. Hence it appears that this product was the anhydrous protoxide of lead. But I found that besides these laminar crystals there were many much smaller crystals adhering to the lead: when examined by the microscope they appeared colourless and semitransparent, having very brilliant facets. Many of them were

* See Dr. M. Hall on the Oxidation of Iron, Journal of Science, vol. vii. p. 55.

perfect rhombic dodecahedrons, and others of the same fundamental form with the acute solid angles replaced by tangent planes: they varied in diameter from about $\frac{1}{200}$th to $\frac{1}{1000}$th of an inch. When heated, these crystals become opake and orange-coloured, but without losing their form or brilliancy of surface. Houton Labillardière*, it is said, obtained dodecahedral crystals of the anhydrous protoxide as a deposit from a solution of oxide of lead in a solution of caustic soda; and Becquerel has obtained cubes by heating the oxide with pure potash†.



9. Lead, as it occurs in commerce, it is well known, generally contains portions of copper and iron. I found on trial that the lead I had used did contain those metals, (it contained no silver,) and traces of copper could even be detected in the grey crystals of oxide, when they were fused with borax before the blowpipe in the reducing flame. Lest the action of water and air described should be in any way dependent on these alloys, I attempted to procure a quantity of the metals quite pure to repeat the experiments on.

10. Nitrate of lead was recrystallized, till the mother liquor gave no trace of copper on the addition of carbonate of ammonia, and the oxide resulting from the calcination of this nitrate was reduced by black flux in a Hessian crucible. It was then kept in fusion for some time at a low red heat in a Wedgwood crucible to separate any carbon it might contain. The lead thus obtained yielded, however, a very slight trace of iron,—derived, I believe, from the action of the flux on the Hessian crucible,—but none of copper. A bright slip of this lead was treated with distilled water as before; the effects were similar: the white crystalline substance formed first, and after about a month, the brilliant grey crystals of anhydrous oxide were formed; they had, perhaps, less of a green shade of colour than those formed on common lead.

11. In a quantity of a solution of oxide of lead in limewater, which had been left a year in a flask stopped by a cork, some brilliant crystalline folia had formed about $\frac{1}{2}$ an inch across, dependent by their upper edges from the surface of the liquid; they were very thin, flexible and elastic. By reflected light, their colour and lustre resembled that of steel blued by heat. They dissolved quietly in acetic acid, became yellow when heated, and appeared identical with the crystallized laminæ before described—anhydrous protoxide (8).

12. A bright iron nail was driven into a clean slip of lead,

* Berzelius, tom. iii. p. 178.

† *Ann. de Chimie*, tom. li. p. 104.

and the combination immersed in a phial of distilled water as before. The next day, the white crystalline substance had formed on the lead, and some rust on the head of the nail: the point in contact with the lead remained bright many days. Another nail, placed in a similar phial of distilled water, for the sake of comparison, was covered by brown hydrated oxide in three days. When the first arrangement was examined after standing seven months, the nail next the lead was still partly bright: the head was covered with rust, mixed with which the grey laminar crystals of oxide of lead and the little dodecahedral crystals were scattered over the surface both of the lead and the iron. The water when tested by sulphuretted hydrogen gave a deep brown tint.

13. A slip of lead, which had the ordinary dull surface which the metal acquires by exposure to the atmosphere, was immersed in a phial of distilled water which had been agitated with air, as in the previous experiments: the water was tested at the end of a week by sulphuretted hydrogen, but gave no indication of holding any lead in solution.

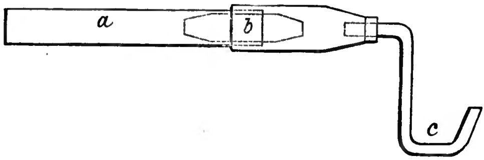
On the white flaky Crystals.

14. Before I consider the nature of this substance, I should observe, that when I made my first experiments on the subject, I was not acquainted with the researches of Dr. Christison, contained in his Treatise on Poisons, p. 458 *et seq.*, 2nd edit., but from my own (5.), I did not doubt that De Morveau was right in concluding that the substance in question was a hydrate. Dr. Christison, however, maintains as the result of his experiments that both the white crystals, whose external character he describes much as I have done, and the lead held in solution by the distilled water, are in the state of carbonate. Most, then, of the experiments that follow were made with the knowledge of Dr. Christison's previous researches, and some with the particular object of determining between G. de Morveau's and my own first opinions on the one hand, and Dr. Christison's results on the other.

15. The substances were obtained as related (8.); the white substance collected and dried *in vacuo* with sulphuric acid: when dry it effervesced very slightly with a dilute acid. A portion weighing 2·67 grains was introduced into a bent tube of green glass closed at one end; the open end of this was fitted into another tube, both being provided with corks and previously counterpoised: the substance was then heated red in the retort tube, and some water condensed in the receiver tube; the tubes were then corked, and weighed when cold: the retort

tube had lost ·28 grain; the water in the receiver weighed ·06 grain only.

16. In another experiment, an open test tube, containing a solution of caustic potash, was placed in the bottle with the lead and water. The white substance in question which formed was dried *in vacuo* as before. A quantity, equal to 1·688 grain, was introduced into a green glass tube about $3\frac{1}{2}$ inches long (*a*), into which was fitted, by grinding, the little tube (*b*), open at both ends, filled with chloride of calcium, and the end of the tube (*c*), which was adapted to the end of tube



(*a*) by a collar of caoutchouc, introduced under a jar full of mercury. The tube (*a*) was then heated: a quantity of gas was given off, equal ·42 cubic inch, of which ·16 cubic inch was absorbed by caustic potash. The tube (*b*) had gained ·05 grain; the tube (*a*) weighed when cold, had lost ·158 grain. Yellow oxide remained.

17. In another experiment with the same apparatus 1·314 grain gave off a portion of gas which was absorbed by potash, but from an accident the quantity was not estimated. The chloride of calcium gained ·036 grain, and the tube (*a*) lost ·154 grain.

18. If the whole loss sustained *minus* the quantity of water collected be reckoned as carbonic acid, we have as the result of the last two experiments:

	Ex. 2.	Ex. 3.
Oxide of lead	1·53	1·16
Water	0·050	0·036
Carbonic acid	0·108	0·118
	1·688	1·314

Or, in 100 parts,	Ex. 2.		Ex. 3.		Ex. 1.
Oxide of lead......	90·63		88·28		89·5
Carbonic acid	6·34	9·3	8·98	11·72	10·5
Water...............	2·96		2·74		

19. These experiments are not sufficiently accordant, nor made on large quantities enough to determine whether the

substance thus obtained is a definite compound or a mixture; but if 223·4 represent two equivalents of oxide of lead, 22·1 carbonic acid, and 9 water, then a substance which should consist of $2\,\dot{P}b + \ddot{C} + Aq$ would contain in 100 parts,

Oxide of lead	87·9	
Carbonic acid	8·6	12·1
Water	3·5	
	100·	

and might also be represented by $\{\dot{P}b + \ddot{C}\} + \{\dot{P}b + Aq\}$.

20. When a portion of this substance was put into a stopped phial with distilled water, and the water tested after some days by sulphuretted hydrogen, it gave a barely perceptible brown tint.

When some of the substance slightly moistened was exposed freely to the air two or three hours, it dissolved in acids with brisk effervescence, like common carbonate of lead.

Of the Solution of Lead in distilled Water.

21. In order to show the power the aqueous solution of oxide of lead possesses of reddening turmeric paper, it is sufficient to put a single slip of fresh-cut lead into a phial of distilled water which has been agitated with air, and introduce with it a bit of turmeric paper: in two or three hours the paper will be permanently reddened.

22. The liquid obtained by experiment 8, and others similarly conducted, has the following properties: 1. it reddens turmeric paper easily, and restores the blue of reddened litmus paper; 2. it gives a deep brown colour with a solution of sulphuretted hydrogen, and soon deposits a black precipitate; 3. it becomes immediately very turbid on the addition of dilute sulphuric acid; 4. also with a solution of carbonic acid; 5. turbid with a solution of sulphate of soda,—the effect much increased by a drop of sulphuric acid; 6. an equal effect with bisulphate of potash. With a drop of a solution of iodide of potassium, it gave a white cloud, which on the addition of a drop of very dilute muriatic acid became orange-yellow, and deposited a yellow precipitate of iodide of lead. With a solution of common salt it immediately becomes turbid; also the same with the solution of sulphate of lime, containing $\frac{1}{1000}$th of that salt; slightly turbid with a solution of nitre.

With neutral chromate of ammonia it preserved its transparency; but when a very minute quantity of acetic acid was added, it deposited a yellow precipitate.

When agitated in a half-filled phial it becomes turbid. The same effect occurs when it is boiled in a glass flask. When left freely exposed to the air without the contact of lead, it deposits the whole, or nearly the whole, of the oxide in the form of the white substance before described.

23. Becquerel has shown that when solutions of lead or manganese are decomposed by the voltaic battery, the conducting wires being of platina, the peroxides of those metals are separated at the positive pole*. I find this to be the case with lead, whether the acetate, the nitrate, or a solution of the oxide in lime water be employed. When the aqueous solution of oxide of lead was thus decomposed, the conducting wires being terminated by slips of platina foil, that at the negative pole was slightly blackened by metallic lead, that at the positive acquired a fine bronze yellow, and the edge of a bit of litmus paper in contact with the foil was bleached.

24. In my first attempts to ascertain the quantity of oxide of lead which was held in solution by distilled water, the liquid obtained, as is described (8.), was filtered; but the filtered liquid was turbid, and when obtained clear by refiltering it was found on testing it that it no longer contained any lead in solution. This was repeated on a portion of liquid from another bottle, which previously to filtering gave a deep brown with sulphuretted hydrogen, with the same effect.

25. To obtain the quantity of oxide of lead dissolved by the water, the rods of lead were carefully removed from the bottle, the stopper replaced, and the liquid, when clear, drawn off by a siphon.

1st trial; 3000 grains of such a solution evaporated with a drop of nitric acid, converted into a sulphate, and thus ignited, weighed ·33 grain = ·242 oxide.

2nd, 3000 grains evaporated with a drop of nitric acid in a flask; the evaporation terminated, and oxide ignited in a platina crucible; the oxide obtained weighed ·245 grain.

3rd, 5000 grains of the solution was divided into two equal parts: one part was reserved for a subsequent experiment; the other, evaporated as in the last, gave ·244 grain of oxide. In the first two trials the quantity of oxide dissolved is about $\frac{1}{12300}$th, in the third $\frac{1}{10250}$.

26. It is very possible, however, that pure water is really capable of holding a greater quantity of oxide of lead in solution than has been here obtained; for it is difficult, if not impossible, to disturb these solutions without occasioning the deposition of part of the oxide in the form of a white powder,

* *Ann. de Chimie*, tom. xliii. p. 380.

e. gr. the removing the rods of lead from the bottle is sufficient. I cannot say from experiment whether this effect is determined by the contact of the carbonic acid in the atmosphere, and that the substance deposited is identical with the white crystalline body already described, or from some other cause, but am more inclined to think it should be referred to the agitation of the liquid, which causes crystallization to take place in a nearly saturated solution.

In an experiment in which fresh distilled water was agitated with pure oxygen gas, clean slips of lead being introduced into it, and the phial closed by a perforated cork, through which an open tube passed filled with fragments of slacked lime, the liquid when examined after three weeks appeared by the action of tests the strongest aqueous solution of lead I had obtained; but the white deposit at the bottom of the phial was much scantier and less bulky, and had none of the crystalline character previously observed. Whether moist or after drying *in vacuo*, it did not present the slightest appearance of effervescence when dissolved by dilute nitric acid.

27. I have before said that Dr. Christison has stated, as the results of his experiments, that the lead dissolved by water is in the state of carbonate. But the following experiments, I think, show, that, in my method of experimenting at least, such is not the case, and also that the carbonate is a much less soluble substance than the oxide or hydrated oxide.

28. A solution of carbonic acid was prepared by passing the gas evolved from calcareous spar and very dilute nitric acid into an air holder, and from that into distilled water, contained in a large two-necked bottle, where the gas could be agitated with the water under moderate pressure. In this way a solution was obtained free from any other acid, and which, by boiling, gave off about two thirds of its volume of carbonic acid gas.

When this solution of carbonic acid was poured into a solution of lead in distilled water, obtained as before (8.), a precipitate took place, which, however, was redissolved by considerable excess of the solution of carbonic acid. In this, again, solution of bicarbonate of potash caused a cloudiness.

29. Into the reserved portion of the solution from the 3rd experiment (par. 22.) = 2500 grains by measure, a current of carbonic acid, evolved from dilute nitric acid and calcareous spar, was passed: this immediately caused a cloudiness, and when the precipitate had subsided, a portion of the clear liquid was tested by sulphuretted hydrogen, which gave a very

pale brown tint. The quantity of oxide actually obtained from the precipitate by igniting and weighing it, amounted to ·132 grain, but part was lost.

30. Some pure protoxide of lead, obtained by heating the subnitrate in a platina crucible, was put into a phial filled with the solution of carbonic acid; another portion of the same oxide was put into a phial filled with distilled water: both phials were closed by ground-glass stoppers and cemented. In the distilled water a quantity of a white flocculent substance formed above the yellow oxide; in the carbonic acid no change could be observed for some time. At the end of a month a small quantity of a white substance could be perceived among the oxide. When the liquids from these arrangements were tested after the lapse of that time, the addition of sulphuretted hydrogen produced no effect in the carbonic acid solution; but it immediately struck a deep brown with the distilled water, which also reddened turmeric, and gave a precipitate with bisulphate of potash and solution of carbonic acid as in prior experiments with metallic lead.

31. Bright slips of lead were put into the solution of carbonic acid (*a*), and to guard against the possibility of the difference of action being owing to any other substance than the carbonic acid, a part of the same solution was boiled for some time to expel the acid, and similar slips of lead introduced into it (*b*). The next day the liquid in (*b*) was milky, in (*a*) the lead continued bright for more than a week; while white crystalline flakes had formed in the other, in which, in fact, the effects were altogether similar to those obtained in previous experiments with distilled water. The liquid from (*a*) gave a barely perceptible tint on the addition of a solution of sulphuretted hydrogen.

On the Carbonate of Lead.

32. Carbonate of lead was prepared by precipitating a solution of the subacetate by a current of carbonic acid gas. The carbonate thus obtained was well washed with distilled water; but when a large quantity had been used, the washings, when tested by sulphuretted hydrogen, still indicated a minute portion of lead to be present: further washing did not diminish the effect. When this had arrived at the minimum, sulphuretted hydrogen gave a pale brown tint; and solution of bicarbonate of potash produced a very faint milkiness after standing some time. When the solution of carbonic acid was passed through carbonate placed on a filter, the filtered liquid gave an immediate milkiness on the addition of bicarbonate of potash,

and with sulphuretted hydrogen a decided brown, but not equal in tint to that produced by distilled water which had been three or four days in contact with lead.

Some of the same carbonate was put into a phial with the solution of carbonic acid and another portion of the carbonate into another phial with distilled water. When the liquids were tested after standing two days, the carbonic acid solution gave a brown colour, the distilled water a very pale tint.

33. To ascertain the degree of solubility of carbonate of lead in the solution of carbonic acid, a solution of carbonate of lead was formed by putting a quantity of carbonate of lead, obtained as before stated, into the solution of carbonic acid in distilled water. After standing some days the clear liquid was drawn off for experiment (α).

Another solution was obtained by washing some good ceruse on a filter, until the filtered liquid, when tested by sulphuretted hydrogen, gave no further diminution of tint. By continued effusions of distilled water, the solution of carbonic acid was then filtered through the washed carbonate, and the liquid preserved for use (β).

34. A standard solution of lead was then made by dissolving one grain of pure oxide of lead in acetic acid, and evaporating this to dryness at the heat of boiling water, and dissolving the resulting acetate in 1000 grains of distilled water.

35. When a portion of the standard solution was diluted with about 83 parts of distilled water, the tint produced on testing this by sulphuretted hydrogen and the solution in carbonic acid (α), was equal. When the standard solution was diluted with between 50 and 60 parts of distilled water, the tint produced on testing it and the solution (β) was equal.

36. Thus it appears that such a solution of carbonic acid as I used is capable of holding in solution about from $\frac{1}{50000}$th to $\frac{1}{60000}$th of oxide of lead if presented to it in the state of carbonate. These solutions immediately become turbid when carbonate or bicarbonate of potash is added; but, with (a) at least, no effect was produced by the action of any of the other tests tried on the distilled water, nor is it made turbid by agitation.

37. When clean cuttings of lead were immersed in solutions of common salt, and sulphate of lime, in the proportions of one part salt to one thousand parts distilled water; in one ounce of water with a drop of sulphuric acid; in the spring water (par. 5.) boiled, and the liquids tested after three weeks, no indication of their holding lead in solution could be perceived.

But the power of saline bodies to prevent the action of pure water on lead is a part of the subject which, noticed

by G. de Morveau, has been so much more fully investigated by Dr. Christison that I shall not dwell further on it. Dr. Christison found that $\frac{1}{30,000}$th of phosphate or hydriodate of potash was an almost perfect preventive of the action of distilled water. This author, however, gives it as his opinion that an unusual quantity of carbonic acid in natural water is the most common counteracting cause which impairs the power of saline bodies to protect lead, and he supports this opinion by an experiment made with the Edinburgh water ("Poisons," p. 465). The few facts that I have observed which bear on this part of the inquiry do not accord with this view of the matter, at least so far as the dissolving power of water is concerned.

38. I mentioned in the first paragraph a spring water which had the power of holding a portion of lead in solution; I found also that when drawn from the spring head, and made to act on lead, as in the before-mentioned experiments, it gave indications in a few days of holding a minute portion of lead dissolved.

A portion of this water was brought from the spring head in a well stopped bottle, and examined. It remained perfectly transparent when an excess of lime was added, and after being boiled 32 measured ounces were evaporated to dryness in a silver crucible at a heat a little above 300°. The residue weighed 1·75 grain: of this $\frac{6}{10}$ths of a grain was soluble in strong alcohol, and proved to be chloride of manganese with chloride of aluminum, and the remainder sulphate of lime with some sulphate of iron and a little silica. Both this spring and the one mentioned rose on the same hill, which consisted of the upper conglomerate beds of the old red sandstone, and contained occasionally thin veins of oxide of manganese.

It is somewhat remarkable that Dr. Lambe* has attributed the power of spring water to dissolve oxide of lead to a "compound salt, the basis of which is manganese and iron, with, perhaps, a little nickel." He thinks that the acid of this salt is the muriatic, and that such a compound is diffused through all common spring water; but this is surely generalizing the matter more than his or any other experiments warrant. It seems clear, however, that he did detect manganese in some of the specimens of spring water which he examined.

On the Electric Relation of Lead to Iron, &c.

39. In the course of these researches I was led to make some experiments on the electrical relation of lead to other

* Researches on Spring Water, p. 158 *et seq.*

metals, particularly iron, partly because it appeared that effects that have been described might be modified by the contact of other metals, and partly because the tables of the electric order of metals given by different philosophers differ from each other in the place assigned to lead. Thus in Dr. Henry's Elements we have first Sir H. Davy's table of metals, the liquid conductor being acid or saline solutions, and it runs thus—zinc, iron, tin, lead, copper, each being positive to those that follow: some pages further in the same work we find Volta's arrangement *, which runs thus—zinc, lead, tin, iron, copper. This also accords with Pouillet's table.

40. My experiments were made with a galvanometer having two needles and a glass thread, as constructed by Dr. Ritchie. The plates of the metals were 2 inches long by 1½ wide, and retained at equal distances by fixing a slip of wood a quarter of an inch thick between them. Copper wires were soldered to the plates to connect them with the cups of the galvanometer. The liquids were contained in cylindrical earthenware cups holding about 4 ounces of water.

I found that when the liquids employed were either distilled water, spring water, solutions of neutral salts, dilute sulphuric, muriatic or nitric acids, lime-water, or solution of caustic potash, and the metals both bright, that lead was to iron constantly as the zinc to copper in the common voltaic arrangements, or that the current was from the lead to the iron through the liquid. But when the lead was tarnished before it was put into the water, or when, being put into acids or saline solutions, it was suffered to remain immersed a short time, the deflection of the needle rapidly diminished, and after a time took place in the opposite direction.

In the Bakerian Lecture for 1826, Sir H. Davy has given an example, and pointed out the general cause, of such reversals in the relations of metals; but the particular case of lead is not there noticed, and it occupies the same relative place in an amended table of the order of metals given in that memoir which it originally held in the series published by the same philosopher in the Journal of the Royal Institution published in 1802.

41. The power of deflecting the needle as measured by the torsion of glass threads given by plates of lead and iron, compared with that given by similar combinations of zinc and copper with the same liquids, is shown approximately in the

* This is given in a memoir by Volta, *Ann. de Chimie*, tom. xl. p. 243, 1802, but he speaks of it as having been previously published.

following results, which probably also show the relative quantities of electricity evolved.

1. Liquid distilled water: lead and iron gave $\frac{1}{10}$th of the force of similar plates of zinc and copper.
2. Liquid spring water used (4. and 5.) $\frac{1}{30}$th. The lead as zinc in both these experiments.
3. Sulphuric acid $\frac{1}{2}$ a drachm, spring water 4 ounces, $\frac{1}{60}$th; the lead being as zinc. In a few minutes the order was reversed; in one instance it was reversed in one minute: the lead became as copper, and the force $\frac{1}{18}$th that given by copper and zinc.

In this state lead was found to be negative even to copper.

With a solution of potash, the lead as zinc, the force was double that given by copper and zinc; and though it diminished rapidly, no reversal took place.

With a solution of carbonic acid in distilled water, no divergence of the needle took place on immersion of the plates of iron and lead.

Conclusion.

The following seem to be the principal conclusions that may be derived from the preceding experiments, and from some facts mentioned by other inquirers.

When lead is immersed in distilled water containing air, the lead combining with oxygen as derived from the air gives rise to the formation of a hydrated oxide, a portion of which, equal to about $\frac{1}{12000}$th of the weight of the water, or a little more, is dissolved (8.), (22.), (25.), (28.);—that besides the lead dissolved, there are formed by the same agency two solid products; the first, in order of time, a very light crystalline substance, which is either a mixture of (18.), or perhaps a compound of (19.), equal proportionals of hydrate and carbonate of lead; the second an anhydrous oxide in grey lamellar crystals, and small white dodecahedrons (8.) That this second substance has crystallized from its aqueous solution is shown by its deposition on the iron (12.).

When small but variable proportions of saline substances are dissolved in the water in which the lead is immersed, the action just mentioned is prevented (Christison); the affinity of water for the oxide of lead appears to be so weakened that no hydrate is formed, and no solution takes place, but the lead is slowly invested by oxide.

But it appears that when exposed to a confined atmosphere loaded with aqueous vapour, and especially if in contact with organic hygrometric substances, it becomes encrusted by carbonate, as in the case of bullets from cartridges, mentioned

by Mr. Faraday*, and the exterior of a water reservoir by Becquerel†.

Most saline solutions, and the sulphuric and carbonic acids, precipitate more or less of the lead from its solution in distilled water, the neutral salts probably in the state of hydrate and the acid salts and free acids in combination (22.).

That carbonic acid dissolved in pure water does not act on lead, or dissolve any measurable portion of its oxide, if at least there be an excess of oxide present, but that it is capable of dissolving a minute portion of the carbonate, though probably such a quantity as is equivalent to less than $\frac{1}{4}$th the oxide which distilled water is capable of taking up (28—36.).

The only spring water I examined which dissolved any oxide of lead was free from carbonic acid (32.).

That with reference to œconomical purposes, it will probably be found that such spring waters as act most on lead, act least on iron, and *vice versâ*.

That in the simple voltaic circle of lead and iron, when both metals are bright, the lead is positive to iron, as Volta originally placed it; but that when the surface of the lead is oxidized, it becomes negative to iron and copper.

12 Duke-street, Grosvenor-square, May 2, 1834. PHILIP YORKE.

XIV. *On the Application of the Principle of Least Pressure to the Theory of Resistances. By the Rev.* H. MOSELEY, *B.A., Professor of Natural Philosophy and Astronomy in King's College, London.*‡

LET P_1 P_2 P_3, &c., represent the different resistances of the system; $\alpha_1\, \beta_1\, \gamma_1$, $\alpha_2\, \beta_2\, \gamma_2$, $\alpha_3\, \beta_3\, \gamma_3$, &c., their inclinations to the axes of $x\, y\, z$; $x_1 y_1 z_1$, $x_2 y_2 z_2$, $x_3 y_3 z_3$, &c., the coordinates of the points of resistance; $u_1 = 0$, $u_2 = 0$, $u_3 = 0$, &c., the relations existing between the coordinates of the several points of resistance, by reason of the connexion of the parts of the system; M_1 M_2 M_3, the sums of the resolved parts of the *given* forces of the system (*i. e.* those which are not resistances) in the directions of three rectangular axes; N_1 N_2 N_3 their moments about those axes.

$$\therefore \quad \Sigma P = \text{minimum}.$$

$$\left.\begin{aligned} \Sigma P \cos\alpha + M_1 &= 0 \\ \Sigma P \cos\beta + M_2 &= 0 \\ \Sigma P \cos\gamma + M_3 &= 0. \end{aligned}\right\} \quad \ldots\ \ldots\ \ldots\ \ldots\ \ldots\ \ldots \quad (1.)$$

* Journal of Science, vol. xvi. p. 163. † *Ann. de Chimie,* tom. liv. p. 146.

‡ Communicated by the Author.

$$\left.\begin{array}{l}\Sigma P\,(y\cos\alpha-x\cos\beta)+N_1=0\\ \Sigma P\,(x\cos\gamma-z\cos\alpha)+N_2=0\\ \Sigma P\,(z\cos\beta-y\cos\gamma)+N_3=0.\end{array}\right\}\quad\ldots\ \ldots\quad(2.)$$

$$\left.\begin{array}{r}u_1=0\\ u_2=0\\ u_3=0\\ \&c.=0.\end{array}\right\}\quad\ldots\ \ldots\ \ldots\ \ldots\ \ldots\quad(3.)$$

$$\left.\begin{array}{l}\mathrm{Cos}^2\alpha_1+\cos^2\beta_1+\cos^2\gamma_1=1\\ \mathrm{Cos}^2\alpha_2+\cos^2\beta_2+\cos^2\gamma_2=1\\ \mathrm{Cos}^2\alpha_3+\cos^2\beta_3+\cos^2\gamma_3=1\\ \qquad\&c. \qquad\qquad\qquad\quad =1.\end{array}\right\}\quad\ldots\ \ldots\quad(4.)$$

Differentiating with respect to $x_1\,y_1\,z_1$, $x_2\,y_2\,z_2$, &c., the resistances of the system being considered functions of these quantities both in respect to their magnitude and direction, we obtain

$$\Sigma\left\{\frac{dP}{dx}\delta x+\frac{dP}{dy}\delta y+\frac{dP}{dz}\delta z\right\}=\delta\Sigma P$$

$$\left.\begin{array}{l}\Sigma\left\{\dfrac{dP}{dx}\cos\alpha\,\delta x+\dfrac{dP}{dy}\cos\alpha\,\delta y+\dfrac{dP}{dz}\cos\alpha\,\delta z-P\sin\alpha\,\delta\alpha\right\}=0\\ \Sigma\left\{\dfrac{dP}{dx}\cos\beta\,\delta x+\dfrac{dP}{dy}\cos\beta\,\delta y+\dfrac{dP}{dz}\cos\beta\,\delta z-P\sin\beta\,\delta\beta\right\}=0\\ \Sigma\left\{\dfrac{dP}{dx}\cos\gamma\,\delta x+\dfrac{dP}{dy}\cos\gamma\,\delta y+\dfrac{dP}{dz}\cos\gamma\,\delta z-P\sin\gamma\,\delta\gamma\right\}=0\end{array}\right\}(1'.)$$

$$\left.\begin{array}{l}\Sigma\left\{\dfrac{dP}{dx}(y\cos\alpha-x\cos\beta)\,\delta x+\dfrac{dP}{dy}(y\cos\alpha-x\cos\beta)\,\delta y\right.\\ \qquad+\dfrac{dP}{dz}(y\cos\alpha-x\cos\beta)\,\delta z+P\cos\alpha\,\delta y\\ \qquad\left.-P\cos\beta\,\delta x-Py\sin\alpha\,\delta\alpha+Px\sin\beta\,\delta\beta\right\}=0\\ \Sigma\left\{\dfrac{dP}{dx}(x\cos\gamma-z\cos\alpha)\,\delta x+\dfrac{dP}{dy}(x\cos\gamma-z\cos\alpha)\,\delta y\right.\\ \qquad+\dfrac{dP}{dz}(x\cos\gamma-z\cos\alpha)\,\delta z+P\cos\gamma\,\delta x\\ \qquad\left.-P\cos\alpha\,\delta z-Px\sin\gamma\,\delta\gamma+Pz\sin\alpha\,\delta\alpha\right\}=0\\ \Sigma\left\{\dfrac{dP}{dx}(z\cos\beta-y\cos\gamma)\,\delta x+\dfrac{dP}{dy}(z\cos\beta-y\cos\gamma)\,\delta y\right.\\ \qquad+\dfrac{dP}{dz}(z\cos\beta-y\cos\gamma)\,\delta y+P\cos\beta\,\delta z\\ \qquad\left.-P\cos\gamma\,\delta y-Pz\sin\beta\,\delta\beta+Py\sin\gamma\,\delta\gamma\right\}=0\end{array}\right\}(2'.)$$

$$\left.\begin{array}{l} u_1'\,\delta x_1 + u_1''\,\delta y_1 + u_1'''\,\delta z_1 = 0 \\ u_2'\,\delta x_2 + u_2''\,\delta y_2 + u_2'''\,\delta z_2 = 0 \\ u_3'\,\delta x_3 + u_3''\,\delta y_3 + u_3'''\,\delta z_3 = 0 \\ \&c. \qquad\qquad = 0 \end{array}\right\} \cdots \cdots \quad (3'.)$$

$$\left.\begin{array}{l} \text{Cos}\,\alpha_1 \sin\alpha_1\,\delta\alpha_1 + \cos\beta_1 \sin\beta_1\,\delta\beta_1 + \cos\gamma_1 \sin\gamma_1\,\delta\gamma_1 = 0 \\ \text{Cos}\,\alpha_2 \sin\alpha_2\,\delta\alpha_2 + \cos\beta_2 \sin\beta\,\delta\beta_2 + \cos\gamma_2 \sin\gamma_2\,\delta\gamma_2 = 0 \\ \text{Cos}\,\alpha_3 \sin\alpha_3\,\delta\alpha_3 + \cos\beta_3 \sin\beta_3\,\delta\beta_3 + \cos\gamma_3 \sin\gamma_3\,\delta\gamma_3 = 0 \\ \&c. \qquad\qquad = 0 \end{array}\right\} (4'.)$$

Hence, adding the above equations, having first multiplied equations (1'.) by the indeterminate quantities $A_1\ A_2\ A_3$ respectively; equations (2'.) by $B_1\ B_2\ B_3$; equations (3'.) by $\lambda_1\ \lambda_2\ \lambda_3\ \lambda_4$, &c. &c.; equations (4'.) by $\mu_1\ \mu_2\ \mu_3\ \mu_4$, &c. &c.

$$\Sigma P = \Sigma \left\{ \begin{array}{l} \left\{ \frac{dP}{dx} \{1 + (A_1 + B_1 y - B_2 z)\cos\alpha + (A_2 - B_1 x + B_3 z) \\ \qquad \cos\beta + (A_3 + B_2 x - B_3 y)\cos\gamma\} \\ \qquad + P(-B_1 \cos\beta + B_2 \cos\gamma) + \lambda u' \right\}.\,\delta x. \\ + \left\{ \frac{dP}{dy} \{1 + (A_1 + B_1 y - B_1 z)\cos\alpha + (A_2 - B_1 x \\ \qquad + B_3 z)\cos\beta + (A_1 + B_2 x - B_3 y)\cos\gamma\} \\ \qquad + P(B_1 \cos\alpha - B_3 \cos\gamma) + \lambda u'' \right\}.\,\delta y. \\ + \left\{ \frac{dP}{dz} \{1 + (A_1 + B_1 y - B_2 z)\cos\alpha + (A_2 - B_1 x \\ \qquad + B_3 z)\cos\beta + (A_3 + B_2 x - B_3 y)\cos\gamma\} \\ \qquad + P(-B_2 \cos\alpha + B_3 \cos\beta) + \lambda u''' \right\}.\,\delta z \\ - P \left\{ (A_1 + B_1 y - B_2 z + \mu\cos\alpha)\sin\alpha\,\delta\alpha + (A_2 - B_1 x \\ \qquad + B_3 z + \mu\cos\beta)\sin\beta\,\delta\beta + (A_3 + B_2 x - B_3 y \\ \qquad + \mu\cos\gamma)\sin\gamma\,\delta\gamma \right\}. \end{array} \right.$$

Now, let the indeterminate quantities $A_1\ A_2\ A_3$, $B_1\ B_2\ B_3$, $\lambda_1\ \lambda_2\ \lambda_3 \ldots \mu_1\ \mu_2\ \mu_3 \ldots$ be taken so as to satisfy the equations of condition (1.) (2.) (3.) (4.); $x_1 y_1 z_1$, $x_2 y_2 z_2$, $x_3 y_3 z_3 \ldots \alpha_1\ \beta_1\ \gamma_1$, $\alpha_2\ \beta_2\ \gamma_2$, $\alpha_3\ \beta_3\ \gamma_3 \ldots$ may then be considered *independent variables*; and by the condition

$$\Sigma P = \text{a minimum},$$

we have

$$\frac{\delta \Sigma P}{\delta x_1} = 0, \quad \frac{\delta \Sigma P}{d y_1} = 0, \quad \frac{\delta \Sigma P}{d z_1} = 0, \quad \frac{\delta \Sigma P}{\delta x_2} = 0,$$

$$\frac{\delta \Sigma P}{\delta y_2} = 0, \quad \frac{\delta \Sigma P}{\delta z_2} = 0, \text{ \&c.; also}$$

$$\frac{\delta \Sigma P}{\delta \alpha_1} = 0, \quad \frac{\delta \Sigma P}{\delta \beta_1} = 0, \quad \frac{\delta \Sigma P}{\delta \gamma_1} = 0, \quad \frac{\delta \Sigma P}{\delta \alpha_2} = 0, \text{ \&c. \&c.}$$

Let $1+(A_1+B_1 y-B_2 z)\cos\alpha+(A_2-B_1 x+B_3 z)\cos\beta+(A_3+B_2 x-B_3 y)\cos\gamma = L$.

$$\left.\begin{aligned} \therefore\quad L\frac{dP}{dx}+P(-B_1\cos\beta+B_2\cos\gamma)+\lambda u' &= 0 \\ L\frac{dP}{dy}+P(\ B_1\cos\alpha-B_3\cos\gamma)+\lambda u'' &= 0 \\ L\frac{dP}{dz}+P(-B_2\cos\alpha+B_2\cos\beta)+\lambda u''' &= 0 \end{aligned}\right\}\dots \quad (5.)$$

$$\left.\begin{aligned} \operatorname{Sin}\alpha\left\{A_1+B_1 y-B_2 z+\mu\cos\alpha\right\} &= 0 \\ \operatorname{Sin}\beta\left\{A_2-B_1 x+B_3 z+\mu\cos\beta\right\} &= 0 \\ \operatorname{Sin}\ \left\{A_3+B_2 x-B_3 y+\mu\cos\gamma\right\} &= 0 \end{aligned}\right\}\dots\dots\dots \quad (6.)$$

Multiplying the first of equations (6.) by $\cos\alpha$, the second by $\cos\beta$, the third by $\cos\gamma$, and adding, observing that

$$\cos^2\alpha+\cos^2\beta+\cos^2\gamma = 0,$$

we obtain

$$(A_1+B_1 y-B_2 z)\cos\alpha+(A_2-B_1 x+B_3 z)\cos\beta+(A_3+B_2 x-B_3 y)\cos\gamma+\mu = 0;$$

$$\therefore\quad L+\mu-1 = 0.$$

$$\text{Let}\qquad \begin{aligned} A_1+B_1 y-B_2 z &= K' \\ A_2-B_1 x+B_3 z &= K'' \\ A_3+B_2 x-B_3 y &= K''' \end{aligned}$$

$\therefore$ by equations (6.)

$$\mu^2 = K'^2+K''^2+K'''^2.$$

Let $\mu . L = K$, $\therefore$ by equations (6.)

$$\frac{\cos\alpha}{L} = -\frac{K'}{K}, \quad \frac{\cos\beta}{L} = -\frac{K''}{K}, \quad \frac{\cos\gamma}{L} = -\frac{K'''}{K}.$$

$\therefore$ by equations (5.)

$$\frac{dP}{dx} + P\frac{B_1K'' - B_2K'''}{K} + \lambda\frac{u'}{L} = 0$$

$$\frac{dP}{dy} + P\frac{-B_1K' + B_3K'''}{K} + \lambda\frac{u''}{L} = 0$$

$$\frac{dP}{dz} + P\frac{B_2K' - B_3K''}{K} + \lambda\frac{u'''}{L} = 0$$

Eliminating λ between these equations

$$\left.\begin{aligned} u''\frac{dP}{dx} - u'\frac{dP}{dy} + P\left\{\frac{B_1(u''K'' + u'K') - K'''(B_2u'' + B_3u')}{K}\right\} &= 0 \\ u'''\frac{dP}{dx} - u'\frac{dP}{dz} + P\left\{\frac{-B_2(u''K''' + u'K'') + K''(B_1u''' + B_3u')}{K}\right\} &= 0 \end{aligned}\right\} (7.)$$

Now, $$\frac{dx}{dz} = -\frac{u'''}{u'}, \quad \frac{dy}{dz} = -\frac{u'''}{u''}$$

$$\frac{dP}{dz} = \frac{dP}{dx}\frac{dx}{dz} + \frac{dP}{dy}\cdot\frac{dy}{dz}$$

$$\therefore \quad \frac{dP}{dz} = -\frac{u'''}{u'}\cdot\frac{dP}{dx} - \frac{u'''}{u''}\cdot\frac{dP}{dy}.$$

Whence, substituting in the second of the above equations (7.), dividing by $\frac{u'''}{u''}$, and adding to the first equation, we obtain an equation of the form

$$\frac{1}{P}\cdot\frac{dP}{dx} + V = 0,$$

where V is a known function of $x\,y\,z$. Eliminating z from this equation by means of the known relation

$$u = 0$$

and integrating, we obtain

$$\log_\varepsilon P + S = Y$$

Where S represents the integral $\int V\,dx$ taken with respect to the variable x, and Y is an arbitrary function of y.

To determine the form of this function, we have

$$\frac{1}{P}\frac{dP}{dx} + V = 0, \quad \frac{1}{P}\frac{dP}{dy} + V' = \frac{dY}{dy},$$

representing the value of $\frac{dS}{dy}$ by V′.

Also eliminating $\frac{dP}{dz}$ equations (7.) assume the form

$$u'' \frac{dP}{dx} - u' \frac{dP}{dy} + P \,.\, R' = 0$$

$$2u'' \frac{dP}{dx} + u' \frac{dP}{dy} + P \,.\, R'' = 0$$

$\therefore$ eliminating $\frac{dP}{dx}$ and $\frac{dP}{dy}$ from these equations

$$-u'' V + u' V' - u' \frac{dY}{dy} + R' = 0$$

$$-2u'' V - u' V' + u' \frac{dY}{dy} + R'' = 0$$

Between which equations if x be eliminated we shall obtain a differential equation of the form

$$\frac{dY}{dy} + Fy = 0$$

where Fy is a known function of y.

$\therefore\ Y = C - \int Fy\,dy = \psi y$ is known

$\therefore\ \log_{\varepsilon} P + S = \psi y$

$\therefore\ P = \varepsilon^{(\psi y - S)}$

The values of P, $\cos\alpha$, $\cos\beta$, $\cos\gamma$, being thus found in terms of $x\ y\ z$, and $P_1\ P_2$, &c., $\cos\alpha_1$, $\cos\alpha_2$, $\cos\beta_1$, $\cos\beta_2$ ... $\cos\gamma_1$, $\cos\gamma_2$, being all supposed to be similar functions of their corresponding coordinates, by substitution in equations (1.) and (2.), we shall obtain six equations determining the values of the six constants $A_1\ A_2\ A_3$, $B_1\ B_2\ B_3$, and thus the solution will be complete.

The coefficients of $\delta\alpha$, $\delta\beta$, $\delta\gamma$ may be written thus:

$$\left\{A_1 + B_1 y - B_2 z + \mu \cos\alpha\right\} . \frac{d\cos\alpha}{d\alpha}$$

$$\left\{A_2 - B_1 x + B_3 z + \mu \cos\beta\right\} . \frac{d\cos\beta}{d\beta}$$

$$\left\{A_3 + B_3 x - B_3 y + \mu \cos\gamma\right\} . \frac{d\cos\gamma}{d\gamma}$$

The equations (6.) will therefore be satisfied by any values of $\alpha\ \beta\ \gamma$ which are independent of $x\ y\ z$. That is, they will be satisfied, provided the inclinations of the resistances to the axes of $x\ y\ z$ be the same for all the points of resistance; that is, provided the resistances be parallel *to one another*, and therefore to the *resultant* of the other forces impressed upon the system.

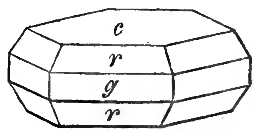
It is *otherwise* manifest, that provided the fixed points on which the system rests be capable of supplying resistance *in every possible direction*, this parallelism is necessary to the condition of a minimum amount of resistance.

When, however, the resistances of the system are supplied by the contact of surfaces capable of motion upon one another, this condition of resistance in every possible direction is not satisfied; the possible directions of resistance at each point lying within the surface of a right cone having that point for its vertex, the normal to it for its axis, and a certain angle dependent upon the friction of the surfaces in contact for its vertical angle.

In this *general* case, then, a parallelism of the resistances is impossible, and we must determine the direction of each according to the principles laid down in the preceding pages, subject to such modifications as may be imposed by the nature of the resisting surfaces and the other conditions of the equilibrium of the system. The discussion of these is reserved for a future Number of this Journal.

XV. *On the crystallized Compounds of* Osmium *and* Iridium, *found in the Ural. By* G. Rose.*

Osmiridium from Newransk. THE inclination of r and c, measured with the reflective goniometer, is 118° nearly. The faces r, c and g possess but little lustre. Cleavage parallel to c, but obtained with difficulty. Colour tin white, rather darker than native antimony. Lustre metallic. Hardness equal to that of quartz. Specific gravity 19·385, obtained by weighing 2·084 grammes of the mineral, selected with great care, in water, at the temperature of 12°·3 Reaumur: a second experiment gave 19·471, the temperature of the water being 9° Reaumur. Heated before the blowpipe on charcoal, it undergoes no change, and does not give the slightest smell of osmium. It smells slightly of osmium when melted with nitre in a matrass, and forms a green mass after cooling. It is found in the gold-sand of Newransk, 95 wersts north of Catharinenburg. Platina occurs with it, but in much smaller quantities. It is also found at Bilimbajewsk, Ryschtim, and many other places in the Ural.



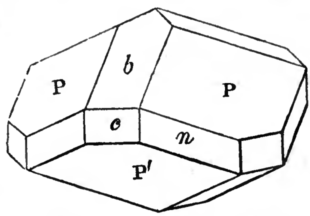
Osmiridium from Nischne Tagil.—The crystals of this variety

* From *Poggendorff's Annalen*, vol. xxix. Part III.

of osmiridium have the same form, angles and cleavage as the preceding. Colour lead-grey, like antimony glance. Hardness that of the preceding variety. Specific gravity of several crystals, which together weighed 1·5205 grammes, 21·118, the temperature of the water being 13° Reaumur. Heated before the blowpipe on charcoal, it does not melt, but the surface becomes dull, and turns black, giving out at the same time a penetrating smell of osmium.

XVI. *On* Phenakite, *a new Mineral from the Ural. By* Nils Nordenskiold.*

THIS mineral is found in Perm, 85 wersts from Catharinenburg. Its form is rhombohedral PP = 115·25, *b* P = 147·42½, *n* P = 122·17½ nearly. Cleavage parallel to *n*. Hardness greater than that of quartz. Specific gravity 2·969. Lustre vitreous. When pure it is transparent and colourless. Sometimes its colour is a bright wine yellow inclining to red: it is also found white and opake. According to Hartwall, it is composed of silica 55·14, glucine 44·47, and traces of alumina and magnesia, amounting, together with the loss, to 0·39. The composition of this mineral is expressed by the formula $\dddot{B\!\!\!-}e + 2\dddot{S}$.



XVII. *On the Influence of the Climate of Naples upon the Periods of Vegetation as compared with that of some other Places in Europe. By* John Hogg, *Esq., M.A., F.L.S. F.C.P.S., &c.*

[Continued from page 50: and concluded.]

III. *Efflorescence.*

FLOWERING (*Efflorescentia*), or the time in which plants unfold their first flowers, is itself also subject to variations no less remarkable than those mentioned in the other periods of vegetation already described.

By comparing the observations reported by Linnæus in his *Philosophia Botanica* upon the flowering of many plants in the neighbourhood of Upsal, and those by M. Chavassieux d'Audibert in the environs of Paris, with my own researches

* From *Poggendorff's Annalen*, vol. xxxiii.

on the same species in the vicinity of Naples, between the periods of efflorescence of these three places in Europe, there result differences so well worth the careful attention of botanists that I have considered it proper to collect them in the following comparative order.

Linnæus in 1748 observed in Upsal the following times of blossoming.

April.—17. Anemone hepatica. 18. Fumaria bulbosa. 22. Tussilago Farfara. 23. Daphne Laureola. 24. Pulmonaria officinalis. 25. Draba verna. 26. Ornithogalum luteum. 27. Viola canina.

May.—1. Ranunculus ficaria. 2. Tussilago Petasites. 3. Lathræa Anblatum. 5. Viola hirta. 6. Primula veris. 7. Glechoma hederacea. 10. Oxalis Acetosella. 15. Draba incana. 16. Leontodon Taraxacum. 17. Saxifraga granulata; Orobus vernus. 18. Adoxa Moschatellina; Alchemilla vulgaris. 19. Chelidonium majus. 24. Pyrus communis. 25. Ranunculus bulbosus. 26. Syringa vulgaris. 28. Anemone Pulsatilla. 29. Empetrum nigrum. 30. Anemone nemorosa.

June.—1. Geum urbanum; Thymus Serpyllum; Bryonia alba; Anchusa officinalis.

From Tenore's Journal of Botanical Observations, the following notices are extracted, relative to the efflorescence of plants near Naples in the year 1800.

Dec.—Leontodon Taraxacum; Narcissus unicolor (*Ten.*); Senecio vulgaris; Bellis perennis.

Jan.—1 to 15. Cardamine hirsuta; Daphne Laureola; Galanthus nivalis; Mercurialis annua; Thlaspi Bursa-pastoris. —16 to 31. Ranunculus ficaria; Fumaria officinalis; F. capreolata; Calendula officinalis; Vinca minor; Anchusa hybrida, (*T.*); Lycopsis bullata; Lamium purpureum; Erodium cicutarium; Alsine media; Veronica Buxbaumii; Euphorbia Peplus; E. helioscopia; Tussilago Farfara; Bellis annua; Ixia minima; Allium Chamæ-moly; Narcissus præcox; Veronica hederæfolia.

Feb.—1 to 15. Vicia Faba; Viola odorata; Synapis nigra; Cynoglossum pictum; Tussilago Petasites; Pulmonaria officinalis; Draba verna; Rosmarinus officinalis; Laurus nobilis; Amygdalus Persica; A. communis; Prunus Cerasus; P. Armeniaca.—16 to 28. Crocus pusillus; Primula acaulis; Narcissus Tazetta; Anemone apennina; Muscari botryoides; Fragaria vesca; Ranunculus Philonotis; R. bulbosus; R. lanuginosus.

March.—1 to 15. Alnus cordifolia; Pyrus Malus; P. communis; Lamium flexuosum (*T.*); Scrophularia peregrina; Linaria officinalis: Glechoma hederacea; Chelidonium majus; Symphytum tuberosum; Borago officinalis; Valantia cruciata. —16 to 31. Cyclamen hederæfolium; Euphorbia sylvatica; Veronica montana; Silene lusitanica; Cerinthe aspera; Coronilla Emerus; Viola canina; Arum italicum; Vicia sativa; Sambucus nigra.

April. — Iris germanica; Allium neapolitanum; Staphylea pinnata; Acer Negundo; Ornithopus compressus; Reseda undata; Ranunculus muricatus; Papaver Rhæas; Lithospermum purpureo-cœruleum; Sanicula europæa; Berberis vulgaris; Robinia Pseudo-acacia; Erysimum officinale; Valeriana rubra; Cratægus monogyna; Lychnis Flos-cuculi; Thymus vulgaris; Euonymus europæus.

May.—Castanea vesca; Vitis vinifera; Plantæ cereales; Rubia tinctorum; Valeriana officinalis; Lavandula Spica; Delphinium peregrinum.

M. Chavassieux d'Audibert noticed the following periods of flowering in the vicinity of Paris.

Jan.—Helleborus niger.

Feb.—Daphne Laureola; Galanthus nivalis; Anemone hepatica; Corylus Avellana.

March.—Viola odorata; Crocus vernus; Primula veris; Tussilago Petasites; Narcissus Tazetta; Prunus Cerasus; Amygdalus communis; A. persica.

April.—Vinca minor; Fragaria vesca; Muscari botryoides; Pyrus Malus; P. communis; P. Cydonia; Syringa persica; Sambucus nigra.

May.—Cytisus Laburnum; Iris germanica; Anchusa officinalis; Symphytum officinale; Borago officinalis; Robinia Pseudo-acacia; Staphylæa pinnata; Berberis vulgaris.

June.—Castanea vesca; Delphinium peregrinum; Papaver album; Vitis vinifera; Lavandula Spica; Thymus vulgaris; Plantæ cereales.

[For the sake of continuing this inquiry in relation to the efflorescence of some of the same plants in England, I will here add the annexed catalogue taken from the Naturalist's Calendar. The plants are arranged alphabetically; and the *mean* time is calculated between the *earliest* and *latest* dates there given, as accurately as may be consistent, without dividing the fraction of a day into hours and minutes.

Flowering.	White at Selborne.			Markwick at Catsfield.		
	Earliest.	*Latest.*	*Mean.*	*Earliest.*	*Latest.*	*Mean.*
Adoxa Moschatellina	March 18.	April 13.	March 31	Feb. 23.	April 28.	March 11
Amygdalus persica...	March 2.	April 17.	March 25	March 4.	April 29.	April 1
Anemone hepatica ...	Jan. 4.	Feb. 18.	Jan. 26	Jan. 17.	April 9.	Feb. 26
Anemone nemorosa...	Mar. 17.	April 22.	April 4	Feb. 27.	April 10.	March 20
Bellis perennis........	Dec. 15.		...	Dec. 26.	Dec. 31.	Dec. 29
Berberis vulgaris.....	May 17.	May 26.	May 21	April 28.	June 4.	May 16
Borago officinalis.....	June 20.		...	April 22.	July 26.	June 8
Bryonia alba	June 9.		...	May 13.	Aug. 17.	June 30
Calendula officinalis..	July 6.	July 9.	July 8	April 20.	July 16.	June 2
Corylus Avellana ...	Jan. 3.	Feb. 28.	Jan. 31	Jan. 21.	Mar. 11.	Feb. 14
Crocus vernus........	Jan. 13.	Mar. 18.	Feb. 14	Jan. 20.	Mar. 19.	Feb. 18
Cytisus Laburnum...	May 18.	June 5.	May 27	May 1.	June 23.	May 27
Daphne Laureola ..	March 25.	April 1.	March 28	April 12.	April 22.	April 17
Draba verna...........	April 14.		...	Jan. 15.	Mar. 24.	Feb. 18
Euonymus europæus	June 20.		...	May 11.	June 25.	June 2
Fragaria vesca.........	April 23.	April 29.	April 26	April 8.	April 9.	April 9
Fumaria bulbosa......	March 19.					
Galanthus nivalis.....	Jan. 10.	Feb. 5.	Jan. 23	Jan. 18.	Mar. 1.	Feb. 8
Geum urbanum......	May 28.		...	May 9.	June 11.	May 25
Glechoma hederacea	April 3.	April 15.	April 9	March 2.	April 16.	March 24
Helleborus niger.....	Jan. 10.		...	April 27.		
Lamium purpureum	Jan. 3.	Jan. 21.	Jan. 12	Jan. 1.	April 5.	Feb. 17
Leontodon Taraxacum	Jan. 16.	Mar. 11.	Feb. 12	Feb. 1.	April 17.	March 10
Lychnis Flos-cuculi...	May 29.	June 1.	May 30	May 12.	June 8.	May 25
Oxalis Acetosella......	March 30.	April 22.	April 10	Feb. 26.	April 26.	March 27
Papaver rhœas	June 24.		...	April 30.	July 15.	June 7
(*Plantæ Cereales.*)						
Secale cereale.......	June 2.		...	May 27.		
Triticum hybernum	June 13.	July 22.	July 2	June 4.	June 30.	June 17
Primula veris.........	April 3.	April 24.	April 13	March 3.	May 17.	April 9
Prunus armeniaca ...	Feb.		...	Feb. 28.	April 5.	March 18
Prunus Cerasus.......	April 18.	May 11.	April 29	March 25.	May 6.	April 15
Pulmonaria officinalis	March 4.	April 16.	March 25	March 2.	May 19.	April 10
Pyrus communis......	April 3.	May 21.	April 27	March 30.	April 30.	April 14
Pyrus Malus...........	April 22.	May 25.	May 8	April 11.	May 26.	May 3
Ranunculus Ficaria..	Feb. 21.	April 13.	March 18	Jan. 25.	Mar. 26.	Feb. 24
Sambucus nigra......	May 26.	June 25.	June 10	May 6.	June 17.	May 27
Sanicula europæa....	May 27.	June 13.	June 4	April 23.	June 4.	May 14
Senecio vulgaris.......	Jan. 3.	Jan. 15.	Jan. 9	Jan. 1.	April 9.	Feb. 19
Symphytum officinale	June 13.		...	May 4.	June 23.	May 29
Syringa vulgaris......	May 21.		...	April 15.	May 30.	May 7
Thlaspi Bursa-pastoris	March 3.		...	Jan. 2.	April 16.	Feb. 23
Thymus Serpyllum...	June 28.		...	June 6.	July 19.	June 27
Tussilago Farfara.....	Feb. 15.	Mar. 23.	March 5	Feb. 18.	April 13.	March 17
Valantia cruciata.....	July 9.		...	April 10.	May 28.	May 4
Valeriana officinalis..	June 22.	July 7.	June 29	May 22.	July 21.	June 21
Veronica hederæfolia	March 1.	April 2.	March 17	Feb. 16.	April 10.	March 14
Vinca minor..........	March 25.		...	Feb. 6.	May 7.	March 23
Viola canina...........	March 6.	April 18.	March 27	Feb. 28.	April 22.	March 26
Viola odorata.........	Feb. 26.	Mar. 31.	March 14	Feb. 7.	April 5.	March 7
Vitis vinifera	June 7.	July 30.	July 3	June 18.	July 29	July 8

But I have also given in one view the times of blossoming of the same plants at Upsal, Naples, and Paris, according to Linnæus, Tenore, and d'Audibert, and the *mean* dates, as in-

cluded in the preceding catalogue, of the same occurrence at Selborne and Catsfield. I have thus arranged them in one list.

Flowering.	Upsal.	Naples.	Paris.	Selborne. (*Mean.*)	Catsfield. (*Mean.*)	England.
Adoxa Moschatellina	May 18	...	...	March 31	March 11	March 21
Amygdalus persica ...	...	Feb. 1—15	March	March 25	April 1	March 28
Anemone hepatica ...	April 17	..	February	Jan. 26	Feb. 26	Feb. 10
Anemone nemorosa..	May 30	...	...	April 4	March 20	March 27
Bellis perennis........	...	December	...	Dec. 15	Dec. 29	Dec. 22
Berberis vulgaris	...	April	May	May 21	May 16	May 18
Borago officinalis.....	...	Mar. 1—15	May	June 20	June 8	June 14
Bryonia alba	June	...	...	June 9	June 30	June 19
Calendula officinalis	...	Jan. 16—31	...	July 8	June 2	June 20
Corylus Avellana ...	...	...	February	Jan. 31	Feb. 14	Feb. 7
Crocus vernus.........	...	...	March	Feb. 14	Feb. 18	Feb. 16
Cytisus Laburnum...	...	...	May	May 27	May 27	May 27
Daphne Laureola ...	April 23	Jan. 1—15	February	March 28	April 17	April 7
Draba verna	April 25	Feb. 1—15	...	April 14	Feb. 18	March 17
Euonymus europæus	...	April	...	June 20	June 2	June 11
Fragaria vesca.........	...	Feb. 16—28	April	April 26	April 9	April 17
Fumaria bulbosa......	April 18	...	...	March 19	...	March 19
Galanthus nivalis.....	...	Jan. 1—15	February	Jan. 23	Feb. 8	Jan. 31
Geum urbanum	June 1	...	...	May 28	May 25	May 26
Glechoma hederacea	May 7	Mar. 1—15	...	April 9	March 24	April 1
Helleborus niger.....	...	...	January	Jan. 10	April 27	March 4
Lamium purpureum	...	Jan. 16—31	...	Jan. 12	Feb. 17	Jan. 30
Leontodon Taraxacum	May 16	December	...	Feb. 12	March 10	Feb. 25
Lychnis Flos-cuculi...	...	April	...	May 30	May 25	May 27
Oxalis Acetosella......	May 10	...	...	April 10	March 27	April 3
Papaver rhœas.........	...	April	...	June 24	June 7	June 15
(*Plantæ Cereales.*)	...	May	June			
Secale cereale........	...	...	...	June 2	May 27	May 30
Triticum hybernum	...	...	...	July 2	June 17	June 24
Primula veris	May 6	...	March	April 13	April 9	April 11
Prunus armeniaca ...	...	Feb. 1—15	...	February	March 18	Feb. 23
Prunus Cerasus	...	Feb. 1—15	March	April 29	April 15	April 22
Pulmonaria officinalis	April 24	Feb. 1—15	...	March 25	April 10	April 2
Pyrus communis......	May 24	Mar. 1—15	April	April 27	April 14	April 20
Pyrus Malus..........	...	Mar. 1—15	April	May 8	May 3	May 5
Ranunculus Ficaria..	May 1	Jan. 16—31	...	March 18	Feb. 24	March 7
Sambucus nigra	...	Mar. 16—31	April	June 10	May 27	June 3
Sanicula europæa ...	...	April	...	June 4	May 14	May 24
Senecio vulgaris	...	December	...	Jan. 9	Feb. 19	Jan. 29
Symphytum officinale	...	...	May	June 13	May 29	June 5
Syringa vulgaris......	May 26	...	...	May 21	May 7	May 14
Thlaspi Bursa-pastoris	...	Jan. 1—15	...	March 3	Feb. 23	Feb. 27
Thymus Serpyllum...	June	...	...	June 28	June 27	June 28
Tussilago Farfara....	April 22	Jan. 16—31	...	March 5	March 17	March 11
Valantia cruciata......	...	Mar. 1—15	...	July 9	May 4	June 6
Valeriana officinalis..	...	May	...	June 29	June 21	June 25
Veronica hederæfolia	...	Jan. 16—31	...	March 17	March 14	March 15
Vinca minor..........	...	Jan. 16—31	April	March 25	March 23	March 24
Viola canina	April 27	Mar. 16—31	...	March 27	March 26	March 27
Viola odorata..........	...	Feb. 1—15	March	March 14	March 7	March 10
Vitis vinifera..........	...	May	June	July 3	July 8	July 5]

From the comparison of these observations, every one will

be able to reckon that the same plants flower in Naples two months and a half (or ten weeks) earlier than at Upsal, and a month (or four weeks) earlier than at Paris.

The considerations before explained respecting the influence of the difference of the seasons in accelerating or in retarding the periods of vegetation, ought in every way to be applied to the time of flowering. In like manner, as in the period of leafing, we may, however, observe that the efflorescence of plants varies from 15 to 20 days, according to the changes in the temperature of the different years.

[Now, before we estimate the times of blowing at Naples with those of the same plants in England, it will be necessary first to reduce each of the dates before given to a more certain medium day between an *early* and a *late* year, by taking 18 days as the *mean varying* time, which, for the purpose of showing the variations in the different Neapolitan seasons, and of being compared with those in the English seasons given in the catalogue at page 105, I have computed in the following manner:

Flowering.	As before at Naples.	A. 18 days earlier.	B. 18 days latter.	Mean of A and B.	True Mean.	England.
Amygdalus persica ..	Feb. 1—15	Jan. 14—28	Feb. 19—Mar. 5	Jan. 21—Feb. 26	Feb. 8	Mar. 28
Borago officinalis ...	Mar. 1—15	Feb. 11—25	Mar. 19—April 2	Feb. 18—Mar. 26	Mar. 8	June 14
Calendula officinalis	Jan. 16—31	Dec. 29—Jan. 13	Feb. 3—18	Jan. 5—Feb. 10	Jan. 23	June 20
Daphne Laureola ..	Jan. 1—15	Dec. 14—28	Jan. 19—Feb. 2	Dec. 21—Jan. 26	Jan. 8	Apr. 7
Draba verna	Feb. 1—15	Jan. 14—28	Feb. 19—Mar. 5	Jan. 21—Feb. 26	Feb. 8	Mar. 17
Fragaria vesca......	Feb. 16—28	Jan. 30—Feb. 11	March 6—18	Feb. 5—Mar. 12	Feb. 22	Apr. 17
Galanthus nivalis ...	Jan. 1—15	Dec. 14—28	Jan. 19—Feb. 2	Dec. 21—Jan. 26	Jan. 8	Jan. 31
Glechoma hederacea	Mar. 1—15	Feb. 11—25	Mar. 19—April 2	Feb. 18—Mar. 26	Mar. 8	Apr. 1
Lamium purpureum	Jan. 16—31	Dec. 29—Jan. 13	Feb. 3—18	Jan. 5—Feb. 10	Jan. 23	Jan. 30
Prunus armeniaca...	Feb. 1—15	Jan. 14—28	Feb. 19—Mar. 5	Jan. 21—Feb. 26	Feb. 8	Feb. 23
Prunus Cerasus.....	Feb. 1—15	Jan. 14—28	Feb. 19—Mar. 5	Jan. 21—Feb. 26	Feb. 8	Apr. 22
Pulmonaria officinalis	Feb. 1—15	Jan. 14—28	Feb. 19—Mar. 5	Jan. 21—Feb. 26	Feb. 8	Apr. 2
Pyrus communis....	Mar. 1—15	Feb. 11—25	Mar. 19—April 2	Feb. 18—Mar. 26	Mar. 8	Apr. 20
Pyrus Malus	Mar. 1—15	Feb. 11—25	Mar. 19—April 2	Feb. 18—Mar. 26	Mar. 8	May 5
Ranunculus Ficaria	Jan. 16—31	Dec. 29—Jan. 13	Feb. 3—18	Jan. 5—Feb. 10	Jan. 23	Mar. 7
Sambucus nigra	Mar. 16—31	Feb. 26—Mar. 13	April 3—18	Mar. 5—Apr. 10	Mar. 23	June 3
Thlaspi Bursa-pastoris	Jan. 1—15	Dec. 14—28	Jan. 19—Feb. 2	Dec. 21—Jan. 26	Jan. 8	Feb. 27
Tussilago Farfara ..	Jan. 16—31	Dec. 29—Jan. 13	Feb. 3—18	Jan. 5—Feb. 10	Jan. 23	Mar. 11
Valantia cruciata ...	Mar. 1—15	Feb. 11—25	Mar. 19—April 2	Feb. 18—Mar. 26	Mar. 8	June 6
Veronica hederæfolia	Jan. 16—31	Dec. 29—Jan. 13	Feb. 3—18	Jan. 5—Feb. 10	Jan. 23	Mar. 15
Vinca minor	Jan. 16—31	Dec. 29—Jan. 13	Feb. 3—18	Jan. 5—Feb. 10	Jan. 23	Mar. 24
Viola canina........	Mar. 16—31	Feb. 26—Mar. 13	April 3—18	Mar. 5—Apr. 10	Mar. 23	Mar. 27
Viola odorata.......	Feb. 1—15	Jan. 14—28	Feb. 19—Mar. 5	Jan. 21—Feb. 26	Feb. 8	Mar. 10

Therefore, from a calculation of the *mean* dates given in the last two comparative Tables, there results this *general* difference in the times of flowering in those countries, that is to say, the efflorescence of the same species of plants occurs

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sooner at Naples by above *seven* weeks, at Paris from *two* to *three* weeks *earlier*, and at Upsal by about *five* weeks *later*, than it is wont to do in England.

But in computing more exactly the times of this cycle of vegetation, little reliance only can be placed on the notices with regard to Upsal and Paris, for Linnæus seems to have made his observations in one single year, and d'Audibert has fixed no precise days of the given months; and moreover they have both neglected to state the number of days which vary in the blossoming of these plants, according to the temperature of the weather in early or late years. On the contrary, the dates given by White and Markwick are the earliest and latest times of flowering for a great many years; and the *mean* of these having been calculated, will establish as accurately as possible the true date for the return of this period of vegetation in England in every ordinary season.]

IV. *Fructescence.*

At the time in which the fruits spontaneously fall, or are easily plucked, off the mother plant, begins that period of vegetation which we distinguish by the name of fructescence (*fructescentia*); to this we may apply the same observations that have been made in the preceding articles upon the variations which the influence of the climates, of the seasons, and of the atmosphere exerts upon the different periods of vegetation. Therefore with us (at Naples) the ripeness of fruits is about 20 days earlier or later, according as the spring and summer have been hotter and more rainy, or drier and more temperate.

Linnæus has observed that *barley* and *wheat* ripen on the 4th of August at Upsal. Near Naples these sorts of corn are reaped in June in Terra di Lavoro and in Puglia; and in July in Abruzzo.

[Owing to the proportionably greater heat that is prevalent in the summer in Sweden, and to the more speedy vegetation than in England, the *wheat* harvest will be found from the annexed view not to be any earlier with us than at Upsal, but to begin at the same time; and *barley* to ripen ten days later with us than in Sweden.

Harvest begins.	Upsal.	Naples.	White at Selborne.			Markwick at Catsfield.			England.
			Earliest.	*Latest.*	*Mean.*	*Earliest.*	*Latest.*	*Mean.*	
Wheat .	Aug. 4	June	July 21	Aug. 23	Aug. 6	July 11.	Aug. 26	Aug. 3	Aug. 4
Barley .	Aug. 4	June	Aug. 1.	Aug. 26	Aug. 13	July 27.	Sept. 4	Aug. 15	Aug. 14
Oats....	...	...	Aug. 1.	Aug. 16	Aug. 8	July 26.	Aug. 19	Aug. 7	Aug. 8]

Cherries* do not ripen at Paris before the latter part of June, whilst at Naples they are eaten from the first week in May.

These facts always confirm the difference of the relations between the periods of vegetation in those parts of the world.

V. *Defoliation.*

When the trees begin to be stripped of their leaves, vegetation finds itself arrived at that its last and mournful period, which botanists call defoliation (*defoliatio*).

This phænomenon, in trees which annually lose the whole of their leaves, takes place in the beginning of the autumn. In evergreen trees the leaves prolong their vegetation beyond the first year, and die after the new ones have already expanded themselves. Although botanists have not paid great attention to the fall of the leaf in evergreen trees, this phænomenon, however, it may be shown, does not happen in periods less constant than those which are observed in trees with annual leaves, concerning which I will limit myself to relate some few comparative notices.

The fall of the deciduous leaves being produced by the torpidness which acts on the motion of the vegetable juices, in consequence of the diminution in the temperature that is felt in the autumnal months, must necessarily be early in cold countries and late in warm climates; and, indeed, similar variations in the years and weather exercise upon defoliation a similar influence to that which we have seen to prevail in the other times of vegetation.

Hence it is that at Upsal, the Hazel, the Ash, the Lime, the Maple, and the Poplar cast their leaves at the first sign of autumn; at Paris these same trees lose them in October, whilst at Naples they keep in full leaf through the whole month of November. The Apple, the Fig, the Elm, the Birch, and the different kinds of Oaks, which at Paris are deprived of their leaves in the beginning of November, near Naples often retain them throughout December. Since, however, the coldness of autumn sometimes takes place very early, as it did in the years 1807 and 1812, the fall of the leaf itself is likewise premature.

[White† observes, on the order in which trees lose their leaves in England,—"One of the first trees that becomes naked is the Walnut: the Mulberry, the Ash, especially if it bears many keys, and the Horse Chestnut come next. All lopped trees, while their heads are young, carry their leaves a long

* According to White, near Selborne Wild Cherries are ripe July 22.

† Works in Natural History, vol. ii. p. 245.

while. Apple trees and Peaches remain green very late, often till the end of November. Young Beeches never cast their leaves till spring,—till the new leaves sprout and push them off: in the autumn the beechen leaves turn of a deep chestnut colour. Tall Beeches cast their leaves about the end of October."]

But it is worth while to state that there grows with us (at Naples) an exotic tree which preserves its deciduous leaves almost to the end of the appearance of the new ones, and consequently seems to confound itself with evergreen trees. This species is the *Salix babylonica*, commonly called Salcio piangente, or Weeping Willow.

[I will now conclude this memoir in again endeavouring to impress strongly upon the minds of my readers the great benefits that may ultimately be bestowed on science by instituting careful inquiries into the changes and periods of vegetation in different situations both at home and abroad, and by comparing the results of their labours in various parts of the globe; and finally, in recalling their attention to the following memorable words of the illustrious Swede:

"Calendaria Floræ quotannis conficienda sunt in quâvis provinciâ, secundum [*Germinationem*,] *Frondescentiam*, [*Efflorescentiam*,] *Fructescentiam*, *Defoliationem*, observato simul *Climate*, ut inde constet diversitas regionum inter se."]

XVIII. *On a remarkable Analogy between ponderable Bodies, and Caloric and Electricity.* *By* William West.*

Some years have elapsed since the attention of chemists was called, by Dulong and Petit, to the relation between the specific heats and the atomic weights of simple bodies. Their experiments and conclusions, though at first called in question, have since been confirmed and extended by others. The relation which they discovered has been variously expressed; it was thus announced by themselves: "The specific caloric of simple bodies is inversely as their atomic weights." It has since been thus stated: "A given quantity of heat will elevate the same number of degrees a portion of every simple substance represented by its atomic weight." A third mode is: "The specific heats multiplied by the atomic weights give a constant quantity." This variety of expression deserves notice, because it may have tended to obscure, if not to conceal, the analogy between caloric and ponderable bodies, which it is my purpose to point out. The analogy in question is this: The quantity of caloric separable from, or combining

* Communicated by the Author.

with, bodies, as measured by their power of imparting or abstracting heat, is exactly proportional to the quantity of any ponderable (simple) substance separable from, or combining with, the same bodies, as ascertained by weighing. Thus it requires, according to the law of Dulong and Petit, twice as much caloric to elevate the temperature of a pound of sulphur 10 degrees, as it does to raise that of a pound of zinc to the same extent; and a pound of heated sulphur will warm twice as much water, or any third substance, as a pound of equally hot zinc. Now this is exactly what takes place among ponderable substances. A certain weight of sulphur combines with twice as much oxygen, or chlorine, as the same weight of zinc does, in forming the correspondent compound and gives up twice as much on entering into a fresh combination. If we were to adapt to the ponderable body oxygen, the phraseology which we are accustomed to use in regard to caloric, we might say then, and the fact would be correct, however unusual the expression, that the "capacity" of sulphur for oxygen is twice as great as that of zinc. Again, "the quantities of oxygen combined with by sulphur and zinc are in inverse ratio to the weights of the atoms of those bodies."

Again, a given quantity of oxygen will raise, in the scale of oxides, portions of zinc and of sulphur represented by their atomic weights. Once more, "the weights of oxygen in the corresponding oxides, multiplied by the atomic weights, give a constant quantity." For 16 parts of sulphur combine with 8 oxygen, 16 zinc with 4 oxygen, but $8 \times 16 = 4 \times 32$. This ground of analogy between caloric and ponderable bodies is so obvious, that it would seem as if it must have occurred to many, yet I have never seen it adverted to. If it has really escaped some who might have been expected to notice it, the difference in our modes of expressing facts thus closely corresponding, when the subject is caloric and when it is a ponderable body, furnishes one reason for the oversight. Another is found in the circumstance that in our experiments and reports of experiments on the relation of bodies to heat, we accustom ourselves to *equal quantities* of the bodies compared, and *variable* quantities of *caloric.*

We are accustomed, on the other hand, to take a fixed quantity of any ponderable substance, and a variable quantity of those whose combinations with it we compare together. But how this analogy has remained without mention is of minor consequence. I wish to draw attention to its existence, and to a fact, exactly corresponding to it in the history of electricity. Mr. Faraday, in his 7th series of researches in that science, says that the quantity of electricity set free during

galvanic action by certain weights of particular bodies, is precisely the same with that which is required to separate them from their combination with other particles when subject to electrolytic action. He describes one experiment in particular, in which "the chemical action of about 32 parts of zinc, arranged as a voltaic battery, was able to evolve a current of electricity capable of decomposing and transferring the elements of 9 grains of water*." Here we may remark the difference in expression, while the fact stated is the same. If for electricity we say iodine, the quantity "set free," or "called into action," during the decomposition of an atom of iodide of zinc, is precisely the same which is required "for forming a new compound on the decomposition of an atom of water." I am aware that the once prevalent doctrine of the materiality of caloric and electricity has given way before the conclusions deduced from certain optical phænomena; but if the being subject to similar laws of combination with material bodies has no weight in restoring them to a place among forms of matter, it may still assist us in investigating others of their relations.

XIX. *On the Internal Structure of Plants.* By the Rev. Patrick Keith, *F.L.S.*†

FROM the previous survey of the vegetable structure‡, it appears that the organs into which the plant is externally distinguishable are the root, trunk, branches, buds, leaves, flower, and fruit, or seed. These we may call *decomposite organs.* But the organs which are thus discoverable by external inspection are each and all of them reducible to component parts, which we will call *composite organs,* and which are again resolvable into constituent parts also, that is, into primary or elementary organs, the detection and exhibition of which are the ultimate object of the researches of the vegetable anatomist.

Decomposite Organs.

The Seed.—In the dissection and anatomy of the seed, no botanist has been so successful as Gærtner. His work *De Seminibus et Fructibus Plantarum,* a work meritorious beyond all praise, is the best guide to the knowledge of carpology, and is to be studied in conjunction with the actual inspection and analysis of seeds. Seeds are divisible into two parts, distinguishable without much difficulty, namely, the integuments

* See Lond. and Edinb. Phil. Mag., vol. iv. p. 294, 295.—Edit.

† Communicated by the Author.

‡ See Lond. and Edinb. Phil. Mag., vol. iii. p. 78, and 120 *et seq.*—Edit.

and *nucleus*, or the embryo and its envelopes. The integuments of seeds are generally two, but sometimes three or four, distinguished in the nomenclature of the present day by the terms, *primine*, *secundine*, *tercine*, *quartine*. The exterior integument, or *primine*, the 'testa' of former anatomists, is the original cuticle of the nucleus, not detachable in the early stages of the seed's growth, but detachable at the period of the maturity of the fruit. The integument next in order is the *secundine*. It originates in the umbilical cord, which, after perforating the testa, or primine, expands either immediately, as if from the *hilum*, or mediately, and from a distinct chalaza, into a multiplicity of ramifications, connected by a fine and delicate membrane that lines the exterior integument, and immediately envelopes the *nucleus*, unless it should happen to be furnished with a tercine.

The nucleus, or kernel, is that part of the seed which is contained within the proper integuments, consisting of the albumen, with the vitellus, when present, and embryo.

The albumen is an organ resembling in its unripe state the white of an egg when raw, and in its ripened state the white of an egg when boiled, and forming for the most part the exterior portion of the nucleus, but always separable from the interior or remaining portion. In the grasses it forms the principal mass of the nucleus and constitutes the *farina* of the seed; but in leguminous plants and in the *Compositæ* it is not at all to be found, at least as a distinct organ.

The *vitellus*, or yelk, where it exists, is an organ of a fleshy but firm contexture interposed between the albumen and embryo, to the former of which it is attached only by adhesion, but to the latter by incorporation of substance. There are many tribes of plants in which its presence cannot be detected, but it pervades the whole of the extensive family of the grasses, where it assumes the form of a scale on which the embryo rests.

The embryo, which is the last and most essential part of the seed and the final object of the fructification, as being the germ or primordial rudiment of the future plant, is a small, pulpy, and very often minute organ, inclosed, for the most part, within the albumen, and occupying very generally the centre of the seed. It is divisible into two distinct and conspicuous parts, namely, the cotyledon and the plantlet.

The cotyledon is that portion of the embryo which incloses and protects the plantlet, and springs up during the process of germination into what is usually denominated the seminal leaf, if the lobe is solitary, or seminal leaves if there are more lobes than one. In the former case the seed is said to be

monocotyledonous, in the latter case it is said to be dicotyledonous, and in the case of its having no cotyledon at all it is said to be acotyledonous. Of plants now known to botanists 6000 species are found to be acotyledonous, 6000 monocotyledonous; and 32,000 dicotyledonous, making a total of 44,000 species * described and arranged according to the prevailing systems; an ample and magnificent Flora of the globe that we inhabit, but not yet complete, for still there are regions of the earth's surface unexplored, and flowers without a name:

.... et sunt sine nomine flores †.

The plantlet is the interior and most essential portion of the embryo, the seat of vegetable life, and the germ that expands into a perfect plant; in short, it is the future plant in miniature. In some seeds it is so minute as to be scarcely perceptible, while in others it is so large as to be divisible into distinct parts. Take and dissect a garden bean, and you will have no difficulty in detecting it. It is situated near the external scar, and is partly lodged within the lobes, and partly in a small and conical process projecting beyond the general line of their circumference and uniting them together. The portion that is lodged within the lobes corresponds to the *caudex ascendens* of Linnæus, being the rudiment of the future leaf and stem, and generally denominated the plumelet; and the portion that is lodged within the conical process corresponds to the *caudex descendens* of Linnæus, being the rudiment of the future root, and generally denominated the radicle.

The Bulb.—If the solid bulb, or what has been called the solid bulb, is taken and divided into halves by a vertical or longitudinal section, it will be found to consist externally of a sort of fibrous or membranaceous envelope, separable into two or more layers; and internally of a fine epidermis, inclosing a firm but succulent pulp, in the centre of which are lodged the rudiments of the future plant. The bulb of *Gladiolus communis* or of *Colchicum autumnale* affords a good example, in which the several parts of the flower may be distinctly perceived long before the period of their natural evolution; yet modern botanists do not admit the existence of a solid bulb; it is now a *coronus*, or *souche souterraine* ‡.

If the coated and scaly bulbs are taken and dissected, they will be found to exhibit similar appearances. Particularly if

* *Cours de Phyt.* par M. Du Petit Thouars.
† *Ovid, Fast.*, lib. iv. p. 441.
‡ Lindley's Introduction to Botany, p. 52.

the bulb of the Tulip is taken up in the beginning of the month of January, and carefully bisected in a line passing through its longitudinal axis, the petals, the stamens, the pistil, and the incipient stem may be already all distinctly perceived, small and delicate in their appearance, but complete in all their parts.

The Bud.—If the scales of a bud are stripped off, and dissected under the microscope, they will be found to consist of a thin epidermis, inclosing a pulp interspersed with a network of fibres. But buds produce either leaves, or flowers, or both, and it was to be presumed that leaves and flowers must exist, in an incipient state, in the bud, long before the period of their natural evolution; which presumption the dissection of buds has shown to be the fact, as the following example will demonstrate. In the month of March 1810, I opened up a bud of the Horse Chestnut that had not yet burst its scales. The scales, which were about fifteen or sixteen in number, being removed, were found to contain one pair of opposite leaves, now laid bare, the divisions of which were closely matted together with a fine down. The leaves upon being opened were found to inclose a flower-spike, consisting of not less than a hundred florets compactly crowded together, and each enveloped by its own downy calyx, which when opened discovered the corolla, stamens and pistil distinct, the rudiments of the future fruit being also discernible in the ovary.

The Flower.—If the calyx or corolla is carefully dissected with the assistance of a good glass, it will be found to consist of the following several and distinct parts: an epidermis, or external envelope; a parenchyma, or soft and pulpy mass; and bundles of longitudinal threads or fibres originating at the base, and subdividing and ramifying throughout the expansion, so as to form a thin and flat network. The stamens and pistils seem to consist merely of a fine epidermis, inclosing a soft and pulpy parenchyma, without exhibiting any traces of longitudinal threads or fibres, or but very rarely so. The filaments of the Tulip are tubular, which is, as I believe, a very rare occurrence.

The Leaf.—The leaf, like the calyx and corolla, is found upon dissection to consist of an epidermis, a parenchyma, and multitudes of interspersed fibres. Take a leaf of common sorrel, and tear it asunder, either in a transverse or longitudinal direction, and fragments of a fine and transparent pellicle will be seen projecting beyond the edge of the torn part. This is the epidermis. When the epidermis is stripped off, the parenchyma appears,—a green and pulpy mass interspersed with the prolongations of the fibres of the

petioles, which are divided into a prodigious number of ramifications mutually embracing and intersecting one another, and forming a fabric similar to a piece of fine network. The principal fibre, extending from the base to the apex of the leaf, forms what is called the midrib; the lateral ramifications form what are called the nerves or veins; terms borrowed from the animal kingdom, but calculated to mislead as having no functions in common with the nerves or veins of animals. Yet many leaves have no transverse fibres. In monocotyledonous plants the fibres are parallel to the midrib. But the most singular circumstance in the structure of the leaf is, not that the fibres are subdivided into a variety of ramifications forming a fine network, but that the network thus formed is double, being actually composed of two distinct layers, the one corresponding to the upper, and the other to the under surface of the leaf. In the leaf of the Orange-tree the network consists of even three layers, as the dissection, or rather the maceration, of that leaf will show, and no language is able to convey an adequate idea of the delicacy and intricacy of the web.

The Caudex, or Mass of the Trunk and Root.—In opening up the caudex, whether ascending or descending, the dissector will soon discover that its internal structure, like its external aspect, or habit, is materially different in different tribes of plants. This was long ago pointed out by Grew in his Anatomy of Trunks, and well illustrated by plates. It was further illustrated by Plumier in his Treatise on the Ferns of America, as also by Linnæus; and still more recently by Messrs. Daubenton and Desfontaines*, who have investigated the subject with great ability, as we cannot but admit, but who by generalizing their notions, perhaps somewhat too hastily, have applied and restricted the modes of organization which they illustrate rather incorrectly to certain tribes of plants. Thus, the two modes of internal structure which they demonstrate and describe are presumed to correspond respectively to monocotyledonous plants on the one hand, and to dicotyledonous plants on the other, the caudex of the latter being represented as composed of distinct concentric and divergent layers, and the caudex of the former as exhibiting merely bundles or assemblages of large, longitudinal, and woody fibres interspersed throughout a pith,—but the fact is, that the two modes of internal structure here specified do not uniformly and respectively pervade the two grand divisions of plants now in question. If all monocotyledonous plants are

* *Mém. de l'Instit. Nat.*, tom. i.

destitute of the aforesaid layers, all dicotyledonous plants are not furnished with them. The caudex of Water-Hemlock (*Cicuta virosa*) exhibits no traces of layers, whether concentric or divergent, though belonging to the dicotyledonous class; and the bulb of the common Onion (*Allium Cepa*) is furnished with concentric layers, at least, though belonging to the monocotyledonous class. In short, there are great difficulties resulting from the adoption of this principle. Where are we to place the *Filices*; where the *Orchideæ*? And from what are we to take our distinctions? From the seed, or from the plant? To these questions no satisfactory answer can be given; and hence it is evident, that if any general division arising from internal structure is to be adopted, it must not be instituted upon the ground of the number of the cotyledons.

As all our classifications of the works of nature are but arbitrary groupings, and are but seldom commensurate with the arrangements of the Divine mind, we can scarcely expect to find a classification that shall be unexceptionable. The following divisions are advanced, not with any pretension to peculiar accuracy or excellence, but merely as exhibiting a general view of that scale of vegetable organization by which plants are found to ascend from the lowest and least perfect, to the highest and most perfect forms*.

I. The caudex a homogeneous and cellular mass, pulpy, powdery, crustaceous, or leather-like, not distinctly divisible into trunk and root, not generally furnished with the several appendages of branch, leaf, and conspicuous flower or fruit; but resolvable merely into an epidermis and an inclosed pulp, or parenchyma.

II. The caudex a symmetrical assemblage of heterogeneous organs, vascular as well as cellular, divisible into trunk and root, furnished for the most part with the several appendages of branch, leaf, and conspicuous flower, or fruit; and resolvable into rind, pulp, and interspersed longitudinal fibre; the cellular structure predominating.

III. The caudex a symmetrical assemblage of heterogeneous organs, vascular as well as cellular, divisible into trunk and root; furnished for the most part with the several appendages of branch, leaf, and conspicuous flower and fruit; and resolvable into bark, wood, and pith; the vascular structure predominating.

The first division comprehends the lowest orders of vegetables, that is, orders exhibiting the fewest traces of organization, the caudex being merely a mass of pulp enveloped in a

* Keith's Phys. Bot., vol. i. p. 286.

fine epidermis. This is the simplest mode of vegetable structure, as has been observed by M. Mirbel in his Anatomy of Plants. It may be exemplified in a variety of species belonging to the class *Cryptogamia*, particularly in the orders *Algæ* and *Fungi.* In the *Algæ* you have a good example of it in *Tremella arborea*, which presents, upon external inspection, the appearance of a sort of irregular mass of wrinkled pulp or jelly, of a brown or reddish colour, adhering to the surface of rotten timber or trunks of decayed trees, without any visible root, and without the appendage of either branch, or leaf, or of conspicuous flower or fruit. If you cut it open it still presents to you merely the appearance of a mass of pulp, or of parenchyma covered with a fine epidermis, that is, of a structure wholly cellular. It is well known to many people who are not botanists by the name of Witches' Butter. In *Tremella Nostoc* you have an equally good example, as also in the several species of *Clavaria*, *Sphæria*, *Peziza*, and many other *Fungi.* But the subjects of examination should be taken when young, for when they are old they are no longer pulpy.

The second division comprehends the middle orders of vegetables, that is, orders exhibiting more manifest traces of organization than the foregoing, the caudex being now partly vascular, as consisting of an epidermis that incloses a voluminous pulp, interspersed with bundles of longitudinal threads or fibres. This mode of structure prevails chiefly in herbaceous and annual, or biennial plants, and necessarily involves some considerable variety, the pulp being sometimes solid and sometimes tubular, and the fibres being in both cases sometimes scattered and sometimes contiguous, sometimes arranged irregularly and sometimes in a determinate order.

The Pulp solid.—If the stipe of *Aspidium Filix-mas* is divided by a transverse section towards the base, it will be found to consist of an epidermis inclosing a firm and consistent pulp, and to exhibit upon the surface of the section five circular spots, of a darker colour than the rest, arranged in a line forming about three fourths of the circumference of a circle, and concentric to the circumference of the stipe. The spots are the divided extremities of five bundles of longitudinal nerves, as may be rendered evident by opening up the stem longitudinally, when the several bundles will appear in the form of strong threads, each surrounded with a proper rind or covering, of a brown colour and membranaceous texture, and extending through the whole length both of the stipe and rachis.

The Pulp tubular. — If the stem of the Garden Parsnep, *Pastinaca sativa*, which constitutes externally a fluted column,

is divided by a transverse section, it will be found to consist of an epidermis, containing a hollow cylinder of pulp, thickly set with bundles of longitudinal fibres disposed in a circular row, and lined with a soft and spongy pith, which is itself tubular, and lined with a fine and transparent membrane, consisting of a most intricate plexus of soft and delicate fibres, forming an ultimate cavity, which is sometimes partly filled up with a fine and cottony down, or with fine and transverse diaphragms resembling cobwebs.

It should be added that the tubular stem does not necessarily form one single and continued cylinder, except in the *Agarics*, but rather a succession of individual cylinders united to one another by joints, or knots, that form transverse diaphragms interrupting the continuity of the tube, even though it is furnished with no pith, as in the grasses. But though the trunk of this order of plants is often tubular, yet the root is not often tubular,—though the root of Water-Hemlock furnishes an exception,—but is wholly filled up with pulp and interspersed fibre, or with layers similar to those of the orders that follow.

The third division comprehends the highest orders of vegetables, that is, orders exhibiting the highest degree of vegetable organization, the caudex being now more decidedly vascular in its structure, as well as more perplexingly intricate in its analysis, as consisting of an outer, an intermediate, and a central part, or, in other words, of a bark, wood, and pith, each having an aspect and texture peculiar to itself.

It has been observed that the progressions of nature are not made *per saltum*, and doubtless there is truth in the remark. The various tribes of plants graduate imperceptibly into one another; and if the arrangements of nature intermingle, so must ours also. Thus, between the first and second of the foregoing divisions there are plants to be found partaking of the character of both, as the dissection of the lobes and peduncle of *Marchantia polymorpha* will evidently show. The lobes are cellular, the peduncle is vascular*. The same thing may be said of the second division, and the division now under our consideration (as coming in sequence). The structure of this last is best exemplified in shrubs and trees. Yet nature does not pass *per saltum* from plants that are purely herbaceous on the one hand, to plants that are purely woody on the other. There is an intermediate order, partaking of the character of either class, that forms the connecting link. In the latter case the wood is perfect, in the former case it is imperfect.

* Keith's Phys. Bot., vol. i. p. 228—344.

The Wood imperfect.—If the root of the Beet (*Beta vulgaris*) is taken, and cut open by a transverse section, it will be found to exhibit a most beautiful example of the union of the concentric and divergent layers as inclosed within a bark and inclosing a pith, and remaining at the same time wholly herbaceous. But the mature stem of the full-grown plant approaches in part to the consistence of wood, and points out its affinity to shrubs and trees.

If the stem of the common Cabbage (*Brassica oleracea*) is divided transversely, it will exhibit, first, a bark; then an inclosed cylinder of a firm and compact texture, interspersed with a multiplicity of divergent rays, and approaching to the consistence of wood; and then a large and firm pulp or pith. The same structure pervades the root also, which is furnished with but little pith. In some stages of the plant's growth the concentric layers are not discernible on the transverse section; but in its stage of decay the divergent layers disappear, and the woody cylinder separates spontaneously into a number of fine and concentric layers resembling lace.

The Wood perfect.—If the trunk of a tree or shrub, such as that of the Oak or Elder, is divided by means of a transverse section, the parts that are peculiarly characteristic of this division will be rendered visible. The outer portion is the bark. In young subjects it is of a flexible and leathery texture, consisting, first, of an epidermis or external pellicle; secondly, of a layer of cellular tissue, called the cellular integument; and thirdly, of a number of thin and concentric layers, designated by the name of the cortical layers, and traversed, at least in aged subjects, by a multiplicity of divergent rays. The intermediate portion of the trunk is the wood, constituting the main body of the full-grown plant, and exhibiting on its horizontal section an indefinite number of concentric layers, intersected by an indefinite number of divergent layers issuing from the centre like the rays of a circle, but often alternating with incomplete rays that do not issue from the centre. The interior portion of the trunk is the pith, a soft and spongy substance, of a whitish colour, lodged in the centre of the wood as in a tube. It is always most abundant in the young shoot, of which it constitutes the principal mass. But as the wood increases, the pith diminishes, and in old and full-grown trunks it totally disappears.

The structure of the branches is similar to that of the trunk, as is also the structure of the root with its divisions, at least till you reach the extreme radicles, which present the appearence of a cylindrical mass of pulp inclosed in a fine epidermis,

and inclosing a firm and longitudinal fibre, or rather a bundle of longitudinal fibres united into one cord, like the filaments that compose the nerves of animals, and which terminate ultimately in soft, bibulous, and club-shaped appendages designated by the appellation of *spongiolæ*.

Thus we trace in plants the foundation of three grand orders, depending upon the relative simplicity or complexity of the structure of the caudex, and presumed to exhaust the subject. They agree in point of number, but not in point of specific extent, with the arrangements of modern botanists, by which plants are reduced to three grand orders depending upon the structure of their seeds—the acotyledonous, the monocotyledonous, and the dicotyledonous. Whether or not these groupings of threes give any support to Professor Burnett's system of triads, I am not able to say; but what we learn, unequivocally, from the foregoing investigation is, that the decomposite organs of all plants, to whatever division belonging, are reducible to one or other of the following constituent parts—epidermis, pulp, pith, cortical layers, ligneous layers, fibre—to the analysis of which we now proceed.

[To be continued.]

XX. *Remarks on* Farey's *Account of the Stratification of the Limestone District of Derbyshire. By* W. Hopkins, *Esq., M.A., Mathematical Lecturer of St. Peter's College, Cambridge, and Fellow of the Geological and Cambridge Philosophical Societies.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

MR. CONYBEARE, in the latter part of his "Inquiry how far De Beaumont's theory is applicable to the mountain chains of this country," which appeared in the Number of the Philosophical Magazine for June, in speaking of the elevated chain of hills which ranges N. and S. through our northern counties, has expressed considerable doubts as to the accuracy of Farey's account of the geology of the limestone district of Derbyshire, which forms the southern extremity of the chain; and this I observe has led you to refer, in a note, to an abstract, published in the Phil. Mag. for January, of an account which I laid before the Cambridge Philosophical Society of an investigation I had recently made of one or two important points in the stratification of that county. During the spring of the present year I have had an opportunity of extending my investigation over the greater part of the district; and as

I have now so fully satisfied myself of the entire erroneousness of Farey's views of its stratification, I will, with your permission, enter with some detail into a refutation of them.

Before I notice those points on which Farey and myself differ so widely, I will mention one in which we are entirely agreed, viz. the regular interstratification of the toadstone with the limestone. The direct evidence of this is afforded by those cliffs in which the fact is matter of observation; and the indirect evidence of it must be sought in the circumstance of our being able to found on this hypothesis a generalization which shall clearly and distinctly embrace all the particular phænomena which the stratification of the district presents to us. The validity of this latter evidence must, of course, depend on that which can be offered in proof of the generalization contended for; but as the examination of this evidence would necessarily involve the complete discussion of the subject, into which it is far from my intention to enter at present, I must content myself with stating my conviction of the fact of this interstratification of the toadstone. Any opinion of its unconformability with the limestone beds, professing to rely upon observation, can only, I think, be founded on a superficial examination or on an imperfect conception of the subject. I mention my conviction on this point more particularly in consequence of Mr. Conybeare's having suggested, in the paper above alluded to, the possible unconformability of the toadstone, from its having, perhaps, been thrust up among the limestone beds at a period posterior to their formation. In addition, however, to the reasons above mentioned, I conceive that any such idea must be completely negatived by the total absence of all indications of that mechanical violence which must necessarily attend the forcible intrusion of a mass of igneous rock among masses previously deposited.

I will proceed, however, to the exposition of what I conceive to be erroneous in Farey's account of the district.

He has asserted (and it should be recollected that on this subject he has made many assertions, but has given no proofs,) that there exist in the limestone district of Derbyshire three distinct beds of toadstone. The limestone occupying the surface, and lying above the *first* or highest toadstone—that between the *first* and *second* toadstones—that between the *second* and *third*—and that beneath the *third*—he has termed respectively the *first*, *second*, *third*, and *fourth* limestones. He states that these beds of toadstone have continuous bassets, that of the third or lowest bed commencing on the north near Castleton, and ranging by Wormhill, Chalmerton, Pike Hall, and the Grange to Bonsal Dale; and those of the two other

beds ranging nearly parallel to the above basset at short distances to the west of it.

Let us first consider the two latter bassets, *i. e.* those of his first and second toadstones. He asserts that they commence on the north at the great fault (which he assumes to run N. and S. between Castleton and Litton,) not far from Windmill Houses, the first passing near the village of Litton and the second near Tideswell. Now of the existence of a toadstone basset anywhere to the north of Litton, where these are said to commence, I have not been able to find the most remote indication, either in my own examination of the spot, or in the information I have derived from the most intelligent miners in the neighbourhood; and moreover I can myself bear positive testimony to the fact that the toadstone near Tideswell, which Farey asserts to belong to the *second* bed, belongs to the same as that at Litton, which he has assigned to the *first*, for I have most distinctly traced the toadstone without the smallest interruption from the one place to the other. It is manifestly brought up by the E. and W. fault which has elevated Litton Edge.

Again, (if I understand him rightly, which is sometimes no easy matter, even with the aid of his able commentator*,) Farey says, that the first basset passes nearly at the southern extremity of Crossbrook Dale, and from thence to Fin Copt Hill; and that the second passes from near Tideswell to the southern extremity of Tideswell Dale next the Wye, ranging thence easterly along the sides of Miller's Dale and Monsal Dale till it descends to the level of the river Wye, at the mouth of Crossbrook Dale, and that crossing the river at that point, it returns westerly along the opposite side of the valley to the top of Priestcliff Lowe.

Now, in the first place, I deny the possibility of tracing any continuous basset from Litton to the south end of Crossbrook Dale, or from the point where the toadstone appears north of Tideswell to the southern extremity of Tideswell Dale; nor is there the slightest indication in either case of any faults by which the bassets between those places respectively might be hidden. In the second place, the toadstone which appears in the southern part of Crossbrook Dale is not the western extremity or basset of the bed to which it belongs, for nothing can be more manifest than that the bed passes across the dale, of which for some distance it occupies the whole of the lower part, ascending in exactly a similar manner on both

* See "Geology of England and Wales," in which Mr. Conybeare has given an exposition of Farey's views far more intelligible than that which Farey himself has given of them.

sides of it to the height (where the upper surface of the bed is most elevated,) of perhaps 200 feet. It has manifestly been raised to this position by an E. and W. fault which ranges nearly parallel to the valley of the Wye, and is here about 300 or 400 yards from it. The toadstone is here seen, from the bottom of the dale to the height just mentioned, to abut directly against the limestone, which forms, as it were, a solid wall intervening between the toadstone and the valley of the Wye, thus concealing the raised edge of the toadstone as we proceed westerly along the latter valley till we come to Litton Mill, where the intervening wall becomes too low for that purpose, and accordingly the toadstone then reappears, its upper surface being high in the side of the valley, at an elevation exactly corresponding to its position in Crossbrook Dale. From this place it is easily traced to the southern extremity of Tideswell Dale, the point at which Farey asserted the toadstone to belong to his *second* bed; whereas the facts I have now stated establish the identity of the beds at Tideswell Dale and Crossbrook Dale as clearly as I have established a similar identity at Litton and Tideswell, as above mentioned.

It must be carefully observed here, that the partial toadstone basset I have just described nowhere descends down the side of Monsal Dale or Miller's Dale to the level of the river, as it must necessarily have done had there been a continuous complete basset crossing the river at the mouth of Crossbrook Dale, as Farey describes; and in like manner on the south of the river the toadstone ranges at a considerable elevation along the side of the valley, as on the opposite side, but nowhere, as far as I have been able to ascertain, does it descend continuously to the margin of the river. I conceive this toadstone to have been elevated by a fault exactly similar to that on the N. side of the river, and exactly parallel to it. According to this view of the subject the valley of the Wye, from Chee Tor to the point near Longstone, must have been originally formed by two parallel faults, 400 or 500 yards asunder, the strata being elevated on the N. side of the more northern, and on the S. side of the more southern fault, leaving the intermediate portion in nearly its original position. In this intermediate portion the lower part of the present valley has been formed, as I conceive, by erosion. The small inclination of the lines of stratification, and their almost unbroken continuity in the precipitous rocks which rise immediately from the margin of the river between Crossbrook Dale and the upper end of Miller's Dale, attest the slight disturbance which the portion of the limestone beds between the

faults has suffered, while on either side of the valley, beyond the faults, we find such undoubted indications of immense dislocations.

The formation of this curious and beautiful vale, cutting completely through the elevated central ridge of the district, perpendicularly to its direction, is thus simply accounted for. I have entered with some detail into the explanation of it for the purpose of comparing it with the necessary deductions from Farey's view of the subject. He asserts, as I have before mentioned, that the basset of the *second* toadstone crosses the Wye at the mouth of Crossbrook Dale *, in which case the limestone forming the N. side of Monsal Dale, immediately E. of this point, must be the *second*, and as such Farey has in fact described it. But this limestone is seen in Crossbrook Dale, at the fault about 300 or 400 yards from the Wye, to abut directly against his *first* toadstone, as above described. Hence we must conclude that the limestone on the *south* side of this fault (instead of that on the *north* side, as I conceive,) has been elevated, and to an immense height, since the *first* limestone and *first* toadstone are no longer superincumbent upon it. And yet it is in this mass of limestone thus elevated above the adjoining part, according to Farey, that the present deep valley of the Wye has been excavated. How the river first selected such a course is most marvellous. The explanation I have myself given of the formation of this valley will at least appear to have the testimony of hydrostatical principles in its favour.

It is almost incredible that so industrious an observer as Farey must have been, should have failed to note any one of the longitudinal faults in the district bordering the Wye, though they are so frequent and characteristic, following almost to mathematical accuracy the law of parallelism. We might suppose from his account of this interesting tract that the stratified masses which it comprises remain, relatively at least to each other, in the regular and undisturbed order in which they were first deposited, although in its external characters we cannot fail to mark the most obvious indications of violent dislocation. This is not, however, the only instance

* A patch of toadstone which is seen at this point at the level of the Wye has, no doubt, led to this erroneous notion. It must not be confounded with that before mentioned in the southern part of Crossbrook Dale as belonging, according to Farey, to his first bed. According to my own view of the subject it belongs to the undisturbed portion of limestone and toadstone lying between the two parallel faults described above. Other patches show themselves at several places along the valley, indicating that this undisturbed portion of the toadstone lies just beneath the bed of the river.

in which our geologist has shown such an aptitude to make "the crooked straight, and the rough places plain."

But to proceed with Farey's imaginary bassets. The first basset passes, he says, from Crossbrook Dale to Fin Copt Hill. I assert in answer, that there is no continuous basset between those points, nor can I find the slightest evidence of any N. and S. fault which might conceal it. From Fin Copt Hill it is said to range south-westerly by Flagg and Moneyash to Gratton Dale near Elton; but on what authority the assertion can possibly rest, I cannot form the most remote idea. I have carefully examined several parts of this range, and have made most diligent inquiry respecting the basset among the most intelligent miners at Moneyash, and have never been able to discover the slightest evidence of its existence between Fin Copt Hill and Gratton Dale. In like manner the supposed second basset is said to range from Priestcliff Lowe west of Moneyash, at a short distance from the first; but where it was conceived to pass over the E. and W. range of hill between Taddington and the basin in which Moneyash is situated, I could never understand either from Farey's work or from Mr. Conybeare's interpretation of it. I have, however, most positive evidence that such is not the line which the basset from Priestcliff Lowe follows, having distinctly traced it from the summit of the Lowe continuously to the E. and immediately to the S. of Taddington to the basset by Chilmerton (considered by Farey as the *third* basset,) on the one hand, and by the W. of Taddington and Blackwell on the other, to the foot of the Lowe, from the summit of which I had set out; thus absolutely demonstrating that the toadstone found at the foot of the Lowe as we descend to the Wye belongs to the same bed as that at its summit, though Farey has asserted the latter to belong to the *second*, and the former to the *third* bed.

I can offer no explanation of these discrepancies on simple matters of fact between Farey's statements and my own. I can only hold myself responsible for the facts I have stated.

From Gratton Dale to Masson Lowe and Matlock High Tor, the bassets I have traced agree more nearly, though still imperfectly as regards the second of them; but in his account of Masson Lowe he has fallen into exactly the same error as at Priestcliff Lowe, and the exposition of it is effected exactly in the same manner. The constitution of this curious hill, however, is too intricate for me now to enter into any description of it*.

* In my first examination of this intricate hill (which I was obliged to make hastily) I conceived the toadstone at Bonsal to be the *first*. A more

It would be useless for me to dwell upon other points in which Farey has fallen into errors exactly similar to those I have already pointed out, and from the same cause—a total neglect of the great distinction between a *partial* and a *complete* basset. Those of which I have more particularly spoken in the vicinity of the Wye are manifestly of the former, instead of being, as he has represented them, of the latter class; and hence we have the important conclusion that the basset immediately to the west of them, from Copt Hill to Chilmerton, must belong to the same and highest bed, instead of being the *third*, as Farey has represented. I have no doubt too of this basset being only a partial one, terminating shortly after crossing the Ashborne and Buxton road, to the south of Chilmerton. Hence, again, I conclude that the limestone west of this line of basset is the *first* limestone, instead of being the *fourth*, according to Farey's notions. The toadstone ranging N. and S. between Dove Hole and the Wye, about two miles E. of Buxton, must, again, be the highest, and the limestone on the surface at this western boundary of the district must have been of contemporaneous formation with that on the eastern side, and passes (in this north-western portion of the boundary which I have more particularly examined) under the shale and grit hills with a gentle dip to the west.

Here, then, Farey and myself are completely at issue respecting that portion of what he calls the great limestone fault which ranges, according to his account, along this western boundary of the district, and which, on his supposition of the *fourth* limestone occupying the surface, must necessarily be estimated at nearly 2000 feet, not merely in particular places, but along every part of its range. Now though I have not yet examined the whole of this boundary with the attention which I hope shortly to devote to it, I have examined a considerable portion of it north of Buxton, and I can only observe (as I have already observed respecting some other statements of Farey's,) that I am totally at a loss to conceive on what evidence his assertions respecting this fault can rest. That partial faults exist along this boundary as in other parts of the district, I doubt not; but I will venture to assert that no evidence can be found of any such continuous fault as that contended for; and I confess that it appears to me most marvellous that Farey should have persisted in a theory which involved as a necessary consequence the existence of this enormous fault, in opposition, as I believe, to all the direct evi-

detailed examination has convinced me that if there *be* two toadstones here, it is the *second*.

dence and obvious appearances in that part of the boundary to which I have more particularly referred.

That a great N. and S. fault, however, does exist, I have no doubt, though inferior in importance and very different in position to that which Farey imagined. It is this fault which has brought to the surface the toadstone basset already mentioned as extending from Copt Hill to the south of Chilmerton, and has produced that central elevated ridge which forms the principal feature in the external character of the district. Of this I believe that I shall be able on some future occasion to offer the most indisputable evidence. Farey's account of the great E. and W. fault at the southern extremity of the limestone agrees with my own observations as far as they have yet extended, which is not far west of Hopton; but whether any N. and S. fault meets this further westward I have not yet ascertained. At the northern extremity of the district, instead of a fault ranging along the whole extreme boundary, an E. and W. one ranges from the north side of Copt Hill, where it meets the great N. and S. fault, to Castleton. That described by Farey as extending from Castleton nearly to Litton, is, I believe, totally unsupported by the slightest evidence.

If we would allow to a geologist on the one hand unlimited power of introducing faults, and on the other an uncontrolled command of denuding agents, he must be greatly wanting in ingenuity if he could not devise almost numberless systems of stratification for a district like Derbyshire. These, however, are not powers to be delegated to any geologist merely for the purpose of supporting his own theory; but we have seen, notwithstanding, how boldly Farey has asserted the existence of faults without reference to the evidence of facts; and in his denuding hypotheses we find a character of still greater hardihood. It is manifest that according to his theory, his *first*, *second*, and *third* limestones, together with his *first*, *second*, and *third* toadstones, must necessarily have been swept away by some means from an extensive tract of country, and this too without leaving a trace of the mighty operation behind. Probably he considered also that the shale and gritstone had shared the same fate; but for all this, and for much more that still continues to puzzle geologists, he conceived he had found an adequate cause in the extraordinary hypothesis of a large satellite revolving at so small a distance from the earth as must have rendered him a most troublesome neighbour; for he accuses him of being, by his powerful attraction, the source of all those disturbances of which such manifest indications still exist on the face of the earth, and

among other things of having carried off the Derbyshire limestones. He appears, according to the same philosopher, to have terminated his existence by precipating himself to the earth, thus, after having long disturbed the peace of his primary, violating in his last act the universal laws of motion*.

Now it is very true that theoretical absurdities are not necessarily associated with imperfect statements of facts, or in all cases with incorrect generalizations of them; and moreover I do not think that geological blunders of five-and-twenty years ago demand any great severity of criticism; but absurdities such as those to which I have just alluded are quite intolerable, even though their date were very long prior to the commencement of the present century, and assuredly inspire us with little reverence for the philosophical opinions of the person who fell into them, or for any particular views with which he associated them.

From the representation I have given of Farey's opinions of the stratification of this limestone part of Derbyshire, it might almost appear that they are scarcely worth the trouble I have taken to refute them. It must, however, be recollected that such refutation can only rest on a much more detailed examination of the district than geologists in general can have an opportunity of giving it, and that, in fact, these opinions have, to a certain degree, stood the test of such cursory investigations as some of our most eminent geologists have made of it; that Farey has been frequently appealed to on this subject during the last twenty years; and moreover that Mr. Conybeare, in the "Geology of England and Wales," (a work distinguished for its general accuracy in the geological details of this country,) whatever doubts he may have entertained as to Farey's accuracy on particular points, has nevertheless considered him sufficient authority for a detailed account of the district in question. For these reasons I shall not, perhaps, appear to have given an undue importance to the refutation of the opinions against which I have been contending.

I shall conclude with a short abstract of the views I have myself formed on the stratification of this limestone district, premising, however, that my examination of some of its details is not yet complete.

1. The toadstone is interstratified with the limestone. It cannot, I conceive, have been forcibly protruded among the limestone beds, but must have been diffused over the *surface* of the country at the time of its emission (assuming it to be of igneous origin).

* See Phil. Mag. for 1807, vol. xxviii.; and for 1808, vol. xxxi.

2. The period of the disturbance or disturbances which produced the present dislocations of the superficial strata must have been long *posterior to that of the production of the toadstone*, since the beds of toadstone have suffered exactly the same dislocation as the limestone.

3. One and the same bed of toadstone pervades the whole district N. of Middleton Moor (by Youlgrave), the limestone above it, occupying the surface, being of contemporaneous formation in every part of it*. There is no valid evidence of the existence of a second bed beneath.

4. In the portion of the district S. of Middleton Moor, I have not yet obtained evidence perfectly conclusive as to there being one or two beds of toadstone. If there be two, it would seem almost necessary that the *lower* one should be contemporaneous with the one above described, the upper one being probably of very limited extent.

5. The principal transverse or N. and S. fault is that already described as extending from Copt Hill to the S. of Chelmerton, the strata on the E. of it being elevated. Many other partial ones, however, exist along the eastern side of the district from Bakewell to Cromford and Wirksworth, and along the north-western boundary. In the former, where the dip is easterly, the E. side is almost without exception the most elevated; and in the latter, where the dip is westerly, the W. side is the elevated one. Of the south-western side I cannot yet speak in detail.

These faults are not distinguished, like the longitudinal E. and W. faults, by strict rectilinearity and persistency in their direction. They are rather formed of a number of partial faults, the directions of which are nearly parallel, and of which the extremities do not exactly meet.

6. The great E. and W. faults are numerous, and *their common direction coincides with the mean dip of the strata.* They are remarkable for their rectilinearity and parallelism to each other.

7. Each of these longitudinal faults is accompanied by one of those fissures which are become so well known to us as mineral veins, this fissure being generally though not universally *on the elevated side of the fault*, to which they are near and parallel.

Conversely, each of the large rake veins, about fifteen or six-

* It was only on renewing my investigation in the spring that I came to this conclusion. Before I had examined Farey's *third* basset, I took for granted its continuity, and therefore regarded it as the complete basset of the *first* toadstone, and consequently the limestone beyond it to the W. as the *second.*

teen in number, which form together the great system of parallel veins characteristic of the district, *is accompanied through at least a considerable part of its course by a fault.*

8. A small system of parallel veins is sometimes found independent of the general system, but having this character in common with it,—*that the common direction of the system coincides with the particular dip of the strata in which it is formed.*

9. Though fissures, I have no doubt, accompany the transverse as well as the longitudinal faults, they much more rarely become good mineral veins, and I know of none that are continuous as such for any considerable distance. Instances, however, are not wanting of productive veins of this class.

10. Many small veins exist in the direction of which I have detected no general law. These *cross* veins, however, are for the most part incomparably smaller as *fissures* than the great E. and W. veins above mentioned.

11. All the great *springs of this district are found in conjunction with the great faults.* I do not at this moment recollect an exception to this rule; for, I believe, in every instance where I observed a powerful spring I had independent evidence of the existence of a great fault. The water is also generally observed to proceed from the upper surface of the toadstone, as might be expected from the circumstance of its being unable to penetrate it. The circumstances attending the positions of these springs, when carefully examined, offer a curious corroboration of the fact of the regular interstratification of the toadstone beds.

12. I have observed no indications of a central point from which the toadstone might be conceived to have flowed as from a crater; nor have I yet observed any very distinct appearances of an altered state of the limestone at its junction with the toadstone. My attention, however, has not yet been so particularly directed to this latter point. Should any of your readers have detected instances of this kind, I should feel obliged for any information respecting them.

We may remark that this view of the subject does not involve, as an essential part of it, the supposition of any great and extensive denudation, but merely that local and partial operation of denuding causes which must be recognised in almost every district.

I remain, Gentlemen, yours, &c.

W. Hopkins.

St. Peter's College, Cambridge,
July 18, 1834.

XXI. Dr. Prout's *Reply to* Dr. W. Charles Henry. *

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN reply to Dr. C. Henry, I take the opportunity of stating, once for all, that I have neither time nor inclination to enter into a controversy on any opinions advanced in my Bridgwater Treatise. That these opinions would provoke discussion I expected, and, indeed, rather wished, as such discussion would be likely to lead to the truth—my great and only object. Hypotheses and theories I care nothing about, further than they are true; and whoever can convince me that any hypothesis which I have published is *not* true, will be esteemed a friend.

It is now nearly twenty years since a paper containing views virtually founded on the hypothesis attacked by Dr. Henry was published by me in your Journal †; and from that time to the present I have seen no reason to doubt its truth. With respect to the facts stated by Dr. Henry, most of them (and, indeed, many more which he has *not* mentioned,) are quite familar to me, as I presume they must be to every one who has attended to the subject. Of his reasoning I shall say nothing. Whether the point in question can, or cannot be legitimately deduced from a comprehensive and *correct* view of the principles, our mathematicians will soon decide. In the mean time, however, I have no hesitation in stating, that such a mass of evidence exists in favour of the hypothesis, (which evidence, if no one else does, I may be induced at some time to arrange and publish,) that nothing but a mathematical demonstration that it *cannot* be true, will at present convince me of its error.

It only remains to state, that I have always adopted the fundamental principle of atomic weights, or definite proportions, established by Dr. Dalton; and have always reflected with pride that this most important doctrine was first taught by an Englishman: but that I never did adopt, and I fear, never shall be able to adopt, some of the details of his "atomic theory." Indeed I have always considered the atomic theory, as explained by Dr. Dalton, far less satisfactory and complete, as a

* See our last Number, p. 33.—EDIT.

† See Annals of Philosophy, Old Series, vol. vi. and vii. (1815 and 1816) "On the relation of the specific gravities of gaseous bodies, and the weights of their atoms." This relation, it need scarcely be stated, is founded on the hypothesis in question, understood, but not expressed.

whole, than his theory of gaseous bodies and of vapours; which, had he done nothing else, would have placed him at the head of modern physical inquirers in this, and in every other country. I remain, Gentlemen, yours, &c.

Sackville-Street, July 18, 1834. W. Prout.

XXII. *An Account of some Experiments on the Electricity of Tourmaline, and other Minerals, when exposed to Heat. By* James D. Forbes, *Esq., F.R.SS. L. & Ed., Professor of Natural Philosophy in the University of Edinburgh**.

ALTHOUGH the phænomena of the pyro-electricity of minerals, as it has been termed, and those of the tourmaline in particular, have, after a long period of neglect, been recently studied by more than one philosopher of eminence, there is a sufficient number of undetermined or debatable points, even at the threshold of the inquiry, to yield facts of novelty and interest to those who will take the trouble to look for them.

Having during the past summer been much engaged in studying the relations of bodies to heat and electricity, I was induced, by having in my possession a considerable number of long tourmalines, to repeat and endeavour to verify some recently published experiments with this mineral. These inquiries brought out several new facts; and, with the hope of adding something to our knowledge in this curious field, I have taken this opportunity of communicating to the Society the results of some very recent experiments.

My attention was principally directed to the verification and extension of the views of M. Becquerel, whose ingenious papers published in the *Annales de Chimie* for 1828, give us almost the only information, with the exception of a short paper by Dr. Brewster published in 1824, which we have gained on this subject since the appearance of the works of Haüy. The undecided state in which several points of the first importance were left by the philosophers of the last century is not a little remarkable. In fact, the answer to the

* From the Transactions of the Royal Society of Edinburgh, vol. xiii. This paper was read before that Society on the 3rd of January 1832: the author explains as follows the delay in its publication:

"The publication of this paper was delayed partly under the idea of prosecuting the experiments of which it contains an account; but the author having engaged in some other researches, which appear to him of more immediate importance, he merely prints this communication in its original form.—April 1834."

fundamental question of whether the tourmaline must be in the act of changing its temperature, in order to the development of electricity, may be considered to rest on the authority of Becquerel (who answered it in the affirmative), former authorities being divided upon it.

Dr. Thomson, in his work on Heat and Electricity, published in 1830, observes, that "when the tourmaline is once excited by heat, it retains its electricity for a long time, if care be taken to place it upon non-conductors. Æpinus found it electric after an interval of six hours*. He adds in a foot-note, "These facts, as stated by Æpinus, if accurate, seem inconsistent with the statement of Canton and Becquerel, that the electricity is only developed whilst the stone is changing its temperature." A statement of Dr. Brewster's might also appear to support the views of Æpinus, and by opposing that of Becquerel, leave the question still undecided. He mentions† that a slice of tourmaline cut transversely to the axis of the crystal, and placed on a plate of glass heated to 212°, adhered to it for six or eight hours, even when the glass was uppermost, the electricity of the tourmaline thus supporting its own weight.

The experiment which I am about to describe will, I think, set at rest the question, and is in fact capable of showing within a few minutes, and in a very pleasing manner, the most essential facts of the relation of the electricity to temperature. M. Becquerel found that when a crystal of tourmaline was heated to 212°, its electricity was inappreciable so long as the temperature remained stationary; but that when placed in a cooler medium, the intensity of the electricity was not, as might have been expected, proportional to the rapidity of the change of temperature, which of course would correspond to the period at which the temperature was highest, but, on the contrary, arose gradually to a maximum, when the tourmaline was about half way cooled to the temperature of the apartment; then gradually diminishing, redescended to zero when it reached that point. This remarkable result M. Becquerel obtained by suspending the crystal horizontally by a fibre of silk under a glass cover, the temperature of the air in which he had the means of regulating; he then applied to the extremities of the crystal, wires from the opposite poles of a dry pile, and, counting the number of oscillations made by the tourmaline, deduced the intensity.

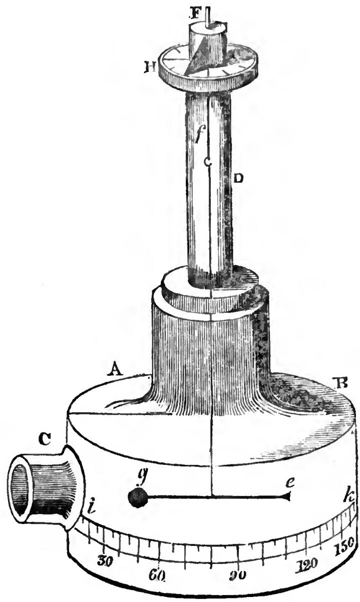
The form of the experiment which I have contrived, and which bears out M. Becquerel's conclusions, gives the same

* p. 478.

† Edinburgh Journal of Science, vol. i. p. 211.

results with great elegance and simplicity, without attempting to indicate the precise temperature of the stone at any period, which, whilst the heat of the medium in which it is placed changes, can only be by M. Becquerel's experiment an approximation, since the interior of the crystal must at any moment have a different temperature from its surface.

I employed a simple form of Coulomb's electrometer, which I constructed for the purpose with little difficulty. A flat-shaped bottle A B, having a wide tubulature at C, was provided. Fitted to the neck is a tube D, plugged at top by a cork F, through which passes a crooked wire *f*, for the purpose of regulating a fibre of raw silk supporting the needle of gum-lac *e*, one end of which is terminated by the disk *g*, of gilt paper. The object to be examined is introduced through the tubulature C, the disk having previously been charged with vitreous or resinous electricity; and the repulsions occasioned by the presence of the body under experiment are measured with sufficient exactness by the deviation of the needle reckoned on the divided circle of paper *i k*. I may add, that the graduated circle H at the top of the glass tube is employed to measure any required torsion of the silk fibre produced by causing the cork F to revolve. By means of this simple apparatus, I obtained results of greater accuracy than my objects generally required*.



Upon presenting to the electrometer, which is quite an insulator, an energetic crystal of tourmaline (which should not

* It is surprising that Coulomb's instrument has not been more employed in this inquiry. Becquerel seems only to have used it once, and Dr. Brewster had recourse to the laborious and unsatisfactory method of causing pyro-electric crystals to lift fragments of the *Arundo Phragmites*, which can give no comparative results.

be too thin), heated to a considerable temperature, the gilt disk being charged with the same electricity as is acquired in cooling by the pole of the crystal presented, the following results appear: At first the tourmaline exerts no influence whatever; but after its temperature has begun to fall, the repulsive power is gradually developed, and the gilt disk slowly recedes as the increasing force appears: the recession becomes very minute, and at length reaches a maximum, at which the needle remains for a time stationary. Soon, however, a diminution of repulsion takes place; the disk reapproaches its original point of rest; and, if left long enough, will return to the zero point precisely opposite the crystal, which then, as at first, produces no action whatever. It is unnecessary to point out how completely this verifies M. Becquerel's views, and demonstrates that, as soon as the temperature completely ceases to change, not the minutest vestige of electricity remains, though the insulation throughout should be maintained as completely as possible. This I generally accomplish by heating and handling the crystal in glass test-tubes.

With regard to Dr. Brewster's remarkable experiment, it partakes, I suspect, of a partly different class of phænomena. It occurred to me that it might, perhaps, if confirmed, be explained thus:—The slice of tourmaline may be considered, in some respect, as an electrical *coating* to the glass. Suppose that the tourmaline and the glass are heated together, and that the side of the slice next the pole of the crystal, assuming vitreous electricity by cooling, is next the glass. Let the other side of the glass (which we shall call the second surface) communicate with the table or any other conductor; by the law of Induction, then, it will assume resinous electricity, the first surface repelling the vitreous. Conceive the glass plate now to be insulated, we shall then have this state of things:—the surface of the tourmaline *furthest* from the glass, by its even excitation, is resinously electrified, for we have supposed the side which coats the first surface of the glass to be vitreous; the resinous electricity, which is insulated at the second surface of the glass, is powerfully attracting the opposite electricity of the side of the tourmaline next itself, and prevents the recombination which would otherwise take place with the electricity of the other side or pole.

Having succeeded in repeating Dr. Brewster's experiment with thin slices from a large crystal of black tourmaline, I found these hypothetical views confirmed. Having heated a slice cut transversely to the axis of the crystal, I laid it upon a plate of cold glass with the side which became vitreously

electric during cooling uppermost; the adhesion was presently complete, so that the glass could be held with the stone suspended from its under surface. The *other* surface of the glass, behind the tourmaline, being then touched with a minute disk of gilt paper, insulated on a thin stick of gum-lac, and then presented to the electrometer, resinous electricity was found of considerable intensity, showing that a decomposition of electricity had actually taken place at the second surface of the glass, the resinously electric pole of the tourmaline forming the coating of its first surface, thus attracting the vitreous electricity of the second surface, and disengaging the resinous. Hence it is easy to see, that if the tourmaline remains sufficiently long warm to prevent the recombination of the electricities of its two poles, until the disengaged electricity at the second surface of the glass shall have been carried off by the air or otherwise, recombination will be prevented, and the electric state will become comparatively permanent.

The use of Coulomb's electrometer, in the manner I have already described, affords an easy and general method of comparing the electric intensities of different crystals. For by measuring the maximum deviation produced by any specimen, we obtain, wholly independent of the exact temperature, a measure of its electric power, a measure independent of time, and, as experience shows, little if at all affected by the precise heat to which the crystal has at first been raised, at least within moderate limits. That the experiment admits, even with the most ordinary attention to collateral circumstances, of considerable accuracy, I have proved by repeating the measures of the intensity of a particular specimen several times in succession. It will at once occur, that a source of fallacy must be guarded against in the loss of the electricity with which the disk of the electrometer is charged, which, as it is constantly diminishing by the contact of air, would give the intensities last measured in a series of comparative experiments too small. In favourable circumstances, and by allowing the disk to remain some time charged before the series is commenced, it is surprising how little this error amounts to. I have always, however, avoided it in practice, by repeating every series of experiments in an inverted order, by which we obtain two observations at equal distances from a mean state of electric tension, the mean of which will give strictly comparable results.

The principal application which I made of this method of observing was to attempt to discover some relation between the form and dimensions of crystals of tourmaline, and their electric power.

M. Becquerel, in a second memoir on the Tourmaline, published in 1828*, announced the rather extraordinary circumstance, that long tourmalines did not become at all electric by heat, and that their facility of being excited was generally inversely proportional to their length. Dr. Thomson, in his work on Heat†, mentions the former assertion, and observes that it is one which he has never had an opportunity of trying. As my experiments have been generally made with black tourmalines from Van Diemen's Land, some of which are of great length, this point early occurred to me as one deserving of investigation. As these inquiries seem at no period to have excited much attention in this country, and as of late nothing whatever has been done upon them, these observations may prove the more interesting.

The longest tourmaline employed by M. Becquerel was six centimetres, or 3·2 English inches, in length, with a diameter of about ·08 inch. My largest tourmaline is 3·25 inches, or almost precisely the same, with a diameter little different. Instead of finding this crystal "tout à fait refractoire," as M. Becquerel describes his, it proved uniformly susceptible of powerful excitement, under the very same treatment which I was accustomed to use towards those of smaller dimensions. The intensity too was very great, though more slowly attained than in shorter ones. Various tourmalines, between two and three inches in length, uniformly show great activity on being removed from the heat to which they have been exposed, and left to cool, when applied to the electroscope.

This discovery led me to some inquiry into the effect of dimension in modifying electric action. Here it is necessary to draw a distinction between the case of excitation and the intensity of the effect produced. M. Becquerel generally mentions the temperature at which electricity appeared: my inquiries have been directed to the maximum intensity of that electricity when excited, which is in some respects the more satisfactory information of the two. The determination of the temperature, we have already seen, is a point of great uncertainty, since every range of atoms, from the centre to the surface, must have a different temperature. Of course, for the reason, the maximum effect is the integral of an infinity of variable forces.

Amongst many experiments on different groups of crystals, I may mention the following as the best determined. Six tourmalines, all 1·3 inch long, whose thicknesses, or areas of section, were represented by the numbers 14, 11, 7, 6, and 4, had

* Ann. de Chimie. † p. 477.

their maximum intensities measured. Two series of experiments in a direct order, and two reversed, all gave the same order of intensity for these specimens, which, instead of bearing any direct proportion to the areas, as might have been expected, where the lengths were equal, gave the following arrangement* in the order of intensities: 1, 2, 5, 4, 3, the areas following the natural order of the numbers. Other series being taken with sets of crystals 1·2 and 1·8 inches long, gave similar indications of irregularity†; but the area of section has so far a general influence, that where the differences are considerable, the thickest crystal has almost universally the greatest power. The relative forces are so connected that we can hardly impute the irregularities to any general law: the differences, as I shall immediately illustrate by reference to another class of experiments, must in all probability be attributed to a variable structure in specimens of the same mineral, as well as in those of different species.

I took a crystal $1\frac{1}{4}$ inch long, and carefully determined the intensity of its electricity, which, by a mean of three experiments, gave 45° of deviation. I immediately broke it at one fourth of its length from one end: the two portions being then heated, and their intensities determined each three times, the mean of the larger portion gave a deviation of 47°, of the smaller 43°, the mean of which gives precisely the original force. As far as intensity goes, the diminution of length would not therefore appear to be favourable to the development of electricity. With a view of procuring through a larger range of dimension the influence of length alone, I selected a series of tourmalines whose sections were as nearly equal as possible, the diameter being about $\frac{1}{10}$th inch, and one of which was the very long crystal before mentioned. This experiment was made with great care; a direct and reversed series were taken, and several of the determinations independently repeated.

* The best pair of series gave,

No. 1,	Deviation 115°
2,	69·5
3,	0
4,	26
5,	39·5

†

Nos.	Intensity. 1·2 long.	Intensity. 1·8 long.
1 (thickest)	82°	54°
2	77·5	40
3	50	34
4	57·5	35·5
5	65	
6 (thinnest)	34	

The mean deviations of the needle of the electroscope will be given in the following Table.

No.	Length.	Intensity.
1	3·25 inch	79°·5
2	2·10	82
3	1·60	60
4	1·55	60
5	1·35	89
6	1·19	68

We thus see that the long crystal holds a high place among those of equal section with it, and we have at the same time an additional proof of the native irregularities of different crystals.

It is well known that the artificial arrangement which represents best the phænomena of the tourmaline, is that of a series of insulated plates of glass arranged parallel to one another, suitably coated, and with the contiguous coatings connected by tinfoil. If one end of this battery be charged from an electrical source, while the other communicates with the ground, the plates at one extremity will partake of an excess of the electricity communicated, whilst those at the other will have the opposite species in excess, and a large proportion of the range in the centre will exhibit no traces of free electricity: hence by shortening the pile (supposing the plates very numerous), no change will take place in the intensity of the free electricity, but the intensity will bear a direct relation to the *surface* of the plates, or the section of the pile. So far analogy supports the increase of intensity with the diameter of the tourmaline; but when we come to consider the mode of charging, it fails, and leaves us in great doubt as to whether the length of a crystal, if its structure be perfectly uniform, should have any influence or not. I have found short crystals of a considerable area, and so formed as to have a large surface, perhaps the most energetic.

The unequal temperature of the portion of any section prevents, as I have already observed, all the parts from giving a maximum intensity at once. This will diminish the total effect, but as all the parts afford the same kind of electricity, the resultant can never be null on this account. Therefore even if the irregularities of amount did not compel us to admit innate varieties of structure, or electric disposition in different specimens, stubborn facts must force us to some such conclusion. In the course of my researches I have met with a crystal of tourmaline * possessing no external irregularities of structure,

* It is No. 3. of the Series at the foot of p. 139.

(the terminations, however, of the crystal are not preserved,) which has the singular property of presenting in cooling a vitreous pole at *both* ends. Having ascertained this point, I proceeded to examine the electricity of its parts by means of Coulomb's *Proof-plane*, by which the electricity of any portion is insulated and examined. As I expected, I found the central portion of the crystal resinously electrified. This remarkable fact is not unexampled. Haüy has recorded the case of a crystal of topaz which had a similar property, which as it is analogous to known facts in the phænomena of magnetism and of double refraction, Dr. Brewster conceived that the crystal of topaz was composed of two with the vitreous poles in contact, as in that case resinous electricity was developed at both ends. Be this as it may, the example of tourmaline which I have cited proves that the junction of the separate crystals, if such exist, may be imperceptible, and as the probability that such irregularity should exist, however caused, is in proportion to the length of the specimen, this may perhaps explain the want of excitability observed by Becquerel in very long crystals.

The phænomena of tourmaline, though entirely electric, bear so strong an affinity to those of magnetism, that the study of their relations must be considered extremely important. I have therefore one remark to make upon an experiment which Dr. Brewster thinks indicative of a "singular breach of analogy between the distribution of the pyro-electrical and magnetical forces." After observing that in the process of reducing a magnet to powder, the coercive force employed effectually destroys all trace of magnetism, he adds, that powder of tourmaline is highly electric when placed on a glass and heated, which is shown by its adhering in conglomerated masses, exhibiting the appearance of viscidity when stirred. It appears to me that this experiment does not go to show that tourmaline *in a state of excitation* does not lose its electricity when bruised in a mortar; indeed, such an experiment it would be impossible to perform. A tourmaline, when it is not changing its temperature, is as inert as a bar of iron before it is magnetized; the process of heating or cooling the one is precisely equivalent to that of conveying magnetism by induction or otherwise to the other. The powder of tourmaline is therefore analogous to the filings of iron, both being equally inert, till the native electricity of the former, and the native magnetism of the latter, is decomposed, when the result in both is perfectly identical.

I shall now only very briefly allude to the conclusions to which some experiments on the electricity of other minerals

besides tourmaline have led me. I have applied Coulomb's electrometer with perfect success to the examination of topaz, boracite, and mesotype, which have all been long known to possess electrical powers. In the case of these minerals, I have been able to extend Becquerel's remarkable law of the intensity of electricity rising to a maximum, when the speed of cooling has become comparatively low, which has not before been demonstrated for any mineral except tourmaline. Topaz possesses the remarkable property of retaining its electricity long after the temperature has ceased to change; probably the decomposition being effected with greater difficulty, the recombination requires more time than in the more excitable minerals. To so great an extent does this take place, that though the maximum deviation of the needle, in one instance amounting to 115°, took place within a few minutes after the excited mineral was presented to the electroscope, in twenty minutes it was hardly diminished, in forty minutes it was still 95°, in an hour 85° *. After a lapse of several hours it was still considerably excited; I obtained similar results with several crystals. Probably in all minerals the difficulty of decomposition and combination increases with the mass; hence, slender crystals are most easily excited, and the effect less permanent. Keeping these facts in view, Æpinus's statement that the tourmaline preserves its electricity, when insulated, for several hours, will admit of easy explanation, by supposing that he worked with large and difficultly excitable crystals, similar to this one of topaz, which had at the same time a very high degree of intensity.

With a large crystal of boracite, having about $\frac{1}{3}$ inch for the side of its cube, I obtained very analogous results. When one of its four resinous poles was presented to the electromter in a warm state, the disk slowly and regularly receded from zero as the cooling advanced, and in about ten minutes reached its maximum duration, which indicated a high degree of intensity. The diminution of electricity was very slow; in three quarters of an hour the disk had receded but a little way. A small crystal of boracite being similarly treated, the maximum was speedily gained, and the needle returned to zero in one experiment in twenty minutes, in another in half an hour. The electricity of the disk in these experiments was extremely steady.

The acicular crystals of mesotype attain with the greatest facility a high degree of electrical excitement, so much so that it required some attention to discover that the maximum inten-

* The disk during this time was of course slowly parting with its charge.

sity was not immediately gained. It lasted a short time, and, as in the case of slender tourmalines, the needle rapidly receded, and in a short time returned to zero.

The very satisfactory results which I have obtained from all these minerals give me great confidence in the aptitude and accuracy of my simple apparatus; and from the very considerable intensity which I find them all to possess, I expect to be able to estimate much smaller degrees of pyro-electricity in other minerals, and in artificial crystals, than have yet been attempted.

Should my first results have appeared interesting to the Society, I may perhaps at no distant time have the honour of communicating my further progress in the inquiry.

Greenhill, January 2, 1832.

XXIII. *Proceedings of Learned Societies.*

ZOOLOGICAL SOCIETY.

1834. *March* 11.—SPECIMENS and drawings were exhibited of a *fresh-water Tortoise,* forming part of the collection of Mr. Bell, by whom it was described as the type of a new genus, for which he proposed the name of

CYCLEMYS.

Sternum latum, testam dorsalem longitudine ferè æquans, integrum, solidum; testæ dorsali ligamento squamato connexum.

CYCLEMYS ORBICULATA. *Cycl. testâ suborbiculari, carinatâ, posticè dentatâ, fuscâ; scutis sterni flavescentibus, fusco radiatim lineatis.*

Long. *dorsi,* 8 unc.; lat. 7; alt. 3.

Emys orbiculata, *Bell.*

Pullus. Emys Dhor, *Gray, Syn. Rept.,* p. 20.?

Hab. in Indiâ.

Mr. Bell regards the *Tortoise* which he has thus characterized as supplying a link in the connecting series of the *land* with the *fresh-water* families which has hitherto been wanting; and as especially valuable in the natural arrangement, by the clue which it furnishes to the correct location of the Indian forms of the genus *Emys.* It is, indeed, most nearly related to *Emys spinosa,* and on a superficial observation might almost be referred to that species; but on closer examination it is found to differ from that *Tortoise,* not only specifically, but generically also: its sternal bones are permanently separated from the dorsal ones, with which they are connected by means of a ligament alone, similar to that which performs the same office in *Terrapene.* From the *Box-Tortoises,* however, to which, in this point of its structure, it is so closely related, *Cyclemys* is altogether distinct, the whole of its *sternum* being entire, instead of having, as is invariably the case in *Terrapene,* one or more transverse divisions of the *sternum* itself, the lobes of which move as on a hinge. In *Terr. Europæa* this mobility of the *sternum* exists in each lobe in a small degree, combined with the ligamentous connexion of the sternal to

the dorsal bones. In *Cyclemys* the whole *sternum* moves together, though very slightly.

The transition from the *land* to the *freshwater Tortoises* may consequently be regarded as commencing in *Terrapene*; passing through *Terr. Europæa* to *Cyclemys orbiculata*; and thence through the Indian forms of *Emys*, which so closely resemble the latter species, to the other forms of *Emys*: the natural series of connexion between the *Testudinidæ* and the *Emydidæ* being thus completed.

The exhibition was resumed of the new species of *Shells* contained in the collection of Mr. Cuming. Those now exhibited were accompanied by characters by Mr. G. B. Sowerby, and consisted of species and varieties additional to those previously characterized by Mr. Broderip, (Lond. and Edinb. Phil. Mag., vol. iii. p. 69.) of the genus CONUS: viz. CON. *Algoensis, Aulicus* (*Var.* roseus), *Nussatella, tendineus, Luzonicus, brunneus, pulchellus, Diadema, ferrugatus,* and *Regalitatis.*

A specimen was exhibited of the *Musk Duck* of New Holland, *Hydrobates lobatus*, Temm. It had recently been presented to the Society by Lieut. Breton, R.N., Corr. Memb. Z. S., who entered into some particulars respecting its habits. He stated that these birds are so extremely rare, that he saw only three of them during his various excursions, which extended over twelve hundred miles of country. He has never heard of any instance in which more than two were seen together. They are met with only on the rivers, and in pools left in the otherwise dry beds of streams. It is extremely difficult to shoot them, on account of the readiness with which they dive; the instant the trigger is drawn, the bird is under water.

Some observations by Dr. Hancock on the *Lantern-fly* and other *Insects* of Guiana were read.

The writer concurs with M. Richard and M. Sieber in regarding as erroneous the statement of Madame Merian, that the *Lantern-fly*, *Fulgora lanternaria*, Linn., exhibits at night a brilliant light, and remarks that the whole of the native tribes of Guiana agree in treating this story as fabulous: it seems to be an invention of Europeans desirous of assigning a use to the singular diaphanous projection, resembling a horn lantern, in front of the head of the insect. He also states that the *Fulgoræ* rarely sing.

The insect whose song is most frequently heard in Guiana is the *Cicada clarisona*, the *Aria-aria* of the Indians, and *Razor-grinder* of the Colonists: in the cool shade of the forests it may be heard at almost every hour of the day; but in Georgetown its song commences as the sun disappears below the horizon. At Georgetown this *Cicada* was never heard in 1804, when Dr. Hancock first visited the place; but it is now very common, probably in consequence of the shelter afforded by the growth of many trees and shrubs in the gardens which have since been formed there. The sound emitted by it is "a long, continuous, shrill tone, which might be compared almost to that of a clarionet, and is little interrupted, except occasionally by some vibrating undulations."

March 25.—A specimen was exhibited of an *Albatross* presented

to the Society by Lieut. Breton, Corr. Memb. Z. S., whose principal object in calling the attention of the Society to it was to mention that, being unprovided at the time at which the bird was killed with any of the ordinary preserving powder or soap, he had used for its preservation a mixture of Cayenne and black peppers with snuff and salt. The skin, well rubbed with this mixture, was brought through the intertropical regions in an ordinary trunk, affording free access to insects, and arrived in England uninjured. Lieut. Breton conceives that it may be advantageous to collectors to be made aware that the preservation of skins can be secured by articles so constantly at hand as those which he employed in this instance.

The exhibition was resumed of the new species of *Shells* forming part of the collection made by Mr. Cuming on the western coast of South America, and among the islands of the South Pacific Ocean. Those brought on the present evening under the notice of the Society were accompanied by characters by Mr. G. B. Sowerby, and consisted of five species of the genus Gastrochæna : viz. Gastroch. *ovata, truncata, brevis, rugulosa,* and *hyalina.*

A Note was read from Mr. Gray, giving an account of the arrival in England of two living specimens of *Cerithium armatum,* which had been obtained at the Mauritius, and had been brought from thence in a dry state. That the inhabitants of *land Shells* will remain alive without moisture for many months is well known : he had had occasion to observe that various marine *Mollusca* will also retain life in a state of torpidity for a considerable time, some facts in illustration of which he had communicated at a recent Meeting of the Society (Lond. and Edinb. Phil. Mag., vol. iii. p. 66.) : the present instance included, however, a torpidity of so long a continuance as to induce him to mention it particularly. The animal, though deeply contracted within the shell, was apparently healthy, and beautifully coloured. It emitted a considerable quantity of bright green fluid, which stained paper of a grass green colour : it also coloured two or three ounces of pure water. This green solution, after standing for twelve hours in a stoppered bottle, became purplish at the upper part ; but the paper retained its green colour though exposed to the atmosphere.

The Secretary mentioned an instance of the arrival in this country of a living *Cerithium Telescopium,* Brug., brought from Calcutta, in company with some small *Paludinæ,* which also reached England alive : these *Mollusca* were, however, kept in sea water frequently changed. The *Cerithium* was placed by Mr. G. B. Sowerby, for dissection, in the hands of the Rev. M. J. Berkeley and G. H. Hoffman, Esq., who have prepared a paper on its anatomy for the forthcoming No. of the 'Zoological Journal' : it will be illustrated by a series of figures, which were exhibited to the Meeting. It is worthy of remark, that the spirit in which this animal was immersed for the purpose of killing it, and in which it was kept for some weeks, became of a dark verdigris colour.

Dr. Weatherhead exhibited two young *Ornithorhynchi* preserved in spirit, which he had recently received from New Holland, and stated his intention of presenting one of them to the Society's Mu-

seum. The smallest of them is about two inches in length; the largest about four. Both are destitute of hair; and in both the eye-lids are closed. In the smaller one there is a vestige of an umbilical slit.

The larger of the two is one of those which were kept in captivity, with their dam, by Lieut. the Hon. Lauderdale Maule, as noticed in a communication read at the Meeting of the Committee of Science and Correspondence of this Society on September 11, 1832, (Lond. and Edinb. Phil. Mag., vol. ii. p. 71.) With it was exhibited the dried skin of the dam, to which the mammary glands, largely developed, had been left adhering.

A Note from Lieut. Breton, Corr. Memb. Z. S., was read, giving an account of an *Echidna*, which lived with him for some time in New Holland, and survived a part of the voyage to England. The animal was captured by him on the Blue Mountains: it is now very uncommon in the colony of New South Wales. He regards it as being of its size the strongest quadruped in existence. It burrows readily, but he knows not to what depth.

Previously to embarkation this individual was fed on ant-eggs and milk, and when on board its diet was egg chopped small with liver and meat. It drank much water. Its mode of eating was very curious, the tongue being used at some times in the manner of that of the *Chamæleon*, and at others in that in which a mower uses his scythe, the tongue being curved laterally, and the food, as it were, swept into the mouth: there seemed to be an adhesive substance on the tongue, by which the food was drawn in. The animal died suddenly off Cape Horn, while the vessel was amidst the ice; perhaps in consequence of the cold, but not improbably on account of the eggs with which it was fed being extremely bad.

Lieut. Breton agrees with MM. Quoy and Gaimard in believing that little difficulty would be experienced in bringing alive to Europe the *Echidna* or *Porcupine Ant-eater* of New Holland. He suggests the following plan.

Previously to embarkation the animal should gradually be weaned from its natural food of ants, which may be done with great facility by giving it occasionally ants and ant-eggs, (the last is, in fact, more properly speaking, its common food,) but more generally milk, with eggs chopped very small, or egg alone. When on board ship it should be kept in a deep box, with strong bars over the top, and a door. It is requisite that the box or cage be deep, because the animal constantly tries its utmost to escape; and possessing very great strength, is liable to injure itself in its exertions to force its way through the bars. The effluvia arising from its excrement are so extremely fetid, that it cannot be kept altogether in a cabin, unless the cage be frequently cleaned. While this is being done, the *Echidna* may be allowed its liberty, but must be narrowly watched, or it will certainly go overboard. It is absolutely necessary that the eggs which are to constitute its food during the voyage be as fresh as possible: they can be preserved in lime water. If milk is not to be procured, water must be supplied daily; and egg and liver (or fresh meat) cut small, should

be given at least every alternate day; but, when the weather will permit, it should be fed once a day. Half an egg (boiled hard) and the liver of a fowl or other bird will suffice for a meal. Finally, the animal should be kept warm, and well supplied with clean straw. It will be as well to nail two or three pieces of wood (battens) across the floor of the cage, to prevent the animal from slipping about when the ship is unsteady.

April 8.—A Letter was read, addressed to the Secretary by John Hearne, Esq., Corr. Memb. Z. S., dated Port au Prince, Feb. 15, 1834. It accompanied a present to the Society of a pair of the *common Goats* of Hayti; referred to various *Birds* which it is the intention of the writer to forward when the season is more advanced; and gave some particulars of a bird known in the island by the name of the *Musicien*, respecting which Mr. Hearne hopes to obtain, in the course of a journey which he projects into the higher lands of the interior, more full information than he at present possesses.

Some extracts were read from a Letter, addressed to Mr. Yarrell by Dr. A. Smith, Corr. Memb. Z. S., dated Cape Town, Jan. 12, 1834. It refers to the projected expedition from the Cape of Good Hope into the interior of Africa, which it is the intention of the writer to accompany. It is designed to proceed directly northward from Latakoo; and Dr. Smith anticipates in this new field numerous additions to his Zoological stores: along the eastern and western coasts he has already penetrated to a considerable distance. Speaking of the *Rodentia*, so numerous in Southern Africa, he mentions as collected by him, in his late visit to Port Natal and the Zoola country, a second species of his genus *Dendromys*. He also notices a new species of *Chrysochloris* obtained by him in the same country.

At the request of the Chairman, Mr. Gould exhibited an extensive series of *Birds* of the genus *Trogon*, Linn., comprising twenty-five species. The greater number of them form part of the Society's Museum, and the others were derived from his own collection.

He pointed out the distinguishing marks of the two sections of the genus, one of which is confined to America, while the other inhabits the Old Continent. He also pointed out among the species exhibited there which he regarded as hitherto undescribed; these he named and characterized as Trogon *erythrocephalus*, *Malabaricus*, and *elegans*.

Mr. Bennett briefly recapitulated the facts and reasonings which have from time to time been brought before the Society on the subject of the abdominal glands of the *Monotremata*, regarded by Meckel and by Mr. Owen as mammary, and by M. Geoffroy-Saint Hilaire as connected with a peculiar function, to which, however, different results have been attributed by that learned zoologist at various times*. The object of the recapitulation was to introduce an abstract of a recent Memoir by M. Geoffroy-Saint-Hilaire, " On the structure and use of the Monotrematic glands, and particularly on those glands

* See Lond. and Edinb. Phil. Mag., vol. i. p. 384; vol. ii. p. 71; vol. iii. pp. 62. 301; vol. iv. p. 54; and the present article, p. 145.

in the *Cetacea.*" In this Memoir the author regards the mammary glands of the *Cetacea*, so analogous in structure to those of *Ornithorhynchus* and *Echidna*, as having a function similar to that which he has attributed to these latter: he assumes that the fluid secreted by them is not milk but mucus, and that this mucus is not sucked by the young, (whose organs of deglutition he describes as being unfitted for sucking,) but is ejected by the mother into the water, the element in which they dwell, where, by imbibition of a portion of the water, it becomes thickened, and, floating by the mother's side, is devoured by the progeny.

M. Geoffroy has subsequently changed his opinion as to the nature of the fluid secreted by the nutrient glands of the *Cetacea*. He had had an opportunity of examining these glands in some *Porpoises*, and had found the secretion to be actually milk. He still, however, maintains that the young of the *Cetacea* do not suck, but that the mother ejects the nutritious fluid from the milk receptacle into the mouth of her young.

April 22.—Some Notes by J. B. Harvey, Esq., Corr. Memb. Z. S., were read: they accompanied a collection of *Shells* and *Crustacea* made by the writer on the coast of Devonshire, near Teignmouth. The several specimens were exhibited.

Among them were numerous individuals of *Cypræa Pediculus*, *Cyp. bullata*, and *Cyp. Arctica*. Of the former there are two varieties, one spotted and the other without spots. The spotted variety, Mr. Harvey states, is generally smaller than the plain one, and is less produced on one side near the *apex*.

Cyp. bullata is found in the same localities as *Cyp. Pediculus*, but it may be doubted whether it is the young of that species: it is so comparatively rare, that Mr. Harvey has dredged up only six specimens of it, while he has collected more than a hundred of *Cyp. Pediculus*: he possesses, moreover, young individuals of *Cyp. Pediculus* of smaller size than specimens of *Cyp. bullata*. In the latter the whorls are more produced at the *apex*, and the shell is so delicate as to be broken by even a slight fall.

On *Cyp. Arctica* Mr. Harvey remarks, that although its size and appearance are in favour of its being a young shell, he hesitates in referring it to the immature condition of the unspotted *Cyp. Pediculus*: his principal ground for doubt is the extreme rarity of *Cyp. Arctica*. He inquires, however, whether the young animal may not, perhaps, live deeply imbedded in the sand for a certain period before it comes to the surface, and thus generally elude the search of the conchologist until its shell becomes matured?

With the *Shells* Mr. Harvey had transmitted to the Society living specimens of *Caryophyllia Smithii*, Brod., the *Torbay Madrepore*, whose habits were described by Mr. De la Beche in the 'Zoological Journal' a few years since: these individuals died on the journey. They are attainable only at the lowest spring tides. They may be kept alive in sea water, changed every second or third day, by feeding them with a very small piece of fresh fish scraped, and deposited with a quill upon the animal, by which it is sucked in in a manner

exactly similar to that of *Polypi*. The colours of some individuals are very vivid; and among these green, blue, and blueish grey are the most predominant. Adhering to the *Caryophyllia* is occasionally found the *Pyrgoma Anglicum*, Leach, which appears to occur in no other situation.

At the request of the Chairman, Mr. Thompson of Belfast exhibited an immature specimen of the *long-tailed Manis, Manis tetradactyla*, Linn., for the purpose of showing that when very young, (the present specimen being but ten inches in length,) the animal is as thoroughly armed, both with respect to scales and spines, as the full-grown one. The specimen was also considered by Mr. Thompson as interesting on account of its locality, it having been obtained in Sierra Leone.

Mr. Thompson also read the following notice of the *Cuckoo, Cuculus canorus*, Linn., copied from his Journal, under the date of 28th May, 1833.

"On examination of three cuckoos to-day, which were killed in the counties of Tyrone and Antrim within the last week, I found them all to be in different stages of plumage: one was mature; another (a female) exhibited on the sides of the neck and breast the reddish-coloured markings of the young bird, the remainder of the plumage being that of maturity; the third specimen had reddish markings disposed entirely over it, much resembling the plumage described by M. Temminck as assumed by 'les jeunes tels qu'ils emigrent en automne', (Man. d'Orn., tom. 1. p. 383,) but having a greater proportion of red, especially on the tail coverts, than is specified in his description of the bird at that age. This individual proved, on dissection, to be a female, and did not contain any eggs so large as ordinary sized peas. The stomach, with the exception of the presence of some small sharp gravel, was entirely empty, and was closely coated over with hair."

Attention was called to the stomach of one of these birds, that the hair with which it is lined might be observed. From its close adhesion to the inner surface of the stomach, and from the regularity with which it is arranged, Mr. Thompson was at first disposed to consider this hair as being of spontaneous growth; but part of the stomach having been subjected to maceration in water, and afterwards viewed through a microscope of high power, the hairs proved, to the entire satisfaction of Mr. Owen and himself, to be altogether borrowed from the *larvæ* of the *Tiger-moth, Arctia Caja*, Schrank, the only species found in the stomach of the bird in various specimens from different parts of the country which were examined by Mr. Thompson in the months of May and June, 1833.

Mr. Thompson also read a Catalogue, with incidental notices, of *Birds* new to the Irish Fauna. He prefaced his list by remarking that he did not bring them forward as unrecorded, without having previously consulted every work in which he was aware that the birds of Ireland are either particularly described or incidentally noticed; including the Statistical Surveys of the Irish counties, which contain, in several instances, Catalogues of the Birds that have been observed in them. The Catalogue is given in the Proceedings of the Society.

Mr. Owen read a Paper "On the Structure of the Heart of the *Perennibranchiate Amphibia*, or *Reptiles douteux* of Cuvier."

He briefly noticed the progressive discoveries relating to the heart of *Reptiles* which have been made since the time of Linnæus, and which have successively rendered inapplicable to the *Saurians, Chelonians*, and *Ophidians*, the phrase "Cor uniloculare, uniauritum", applied to the whole of the *Reptilia* in the '*Systema Naturæ*'. He alluded to the researches of Dr. Davy and M. Martin St. Ange on the structure of the heart in the *Caducibranchiate Amphibia*, from which it appeared that two auricles were appended to the ventricle in those *Reptiles*, as well as in the higher orders above mentioned. He then proceeded to give the results of an examination of the hearts of specimens of *Amphiuma*, Cuv., *Menopoma*, Harlan, *Proteus*, Schreib., and *Siren*, Linn. He selected the heart of the *Siren lacertina* as the subject of detailed description, considering that the genus *Siren*, in combining with persistent external *branchiæ* a limited number of extremities, exhibits the simplest form of the *Amphibious Reptile*.

The heart in this species consists of three distinct cavities, as in the higher *Reptilia*, viz. of two auricles and one ventricle. The auricles appear to form externally one large and remarkably fimbriated cavity, situated behind, and advancing forwards, on both sides of the ventricle and *bulbus arteriosus*. The venous blood is poured into a large membranous sinus by one posterior and two anterior *venæ cavæ* prior to passing into the auricle. The conjoined trunk of the pulmonary veins appears also to enter this sinus, but it passes through without communicating with that cavity, and terminates in a small separate auricle, which opens into the ventricle by an orifice distinct from, but close to, the orifice of the right auricle. In the ventricle a rudimentary *septum* was noticed as affording an indication of a type of formation superior to that of *Fishes*. In the *bulbus arteriosus* a longitudinal projection appears as a commencing division of the single artery, which is given off from the ventricle.

The differences in the structure of the preceding parts, and in the origin and distribution of the different vessels exhibited by the other genera of *Perennibranchiata*, were successively noticed; and the affinities indicated by these modifications to the *Caducibranchiate Reptiles* on the one hand, and to the *Cartilaginous Fishes* on the other, were also pointed out.

The Paper was illustrated by drawings of the structures described in it.

XXIV. *Intelligence and Miscellaneous Articles.*

ON SOME NEW COMBINATIONS OF PLATINA. BY M. DŒBEREINER.

IF solutions of muriate of platina and carbonate of soda, the latter being in excess, be mixed and exposed for some days to the sun, or a temperature of 212°, there is gradually formed a precipitate, of a chrome yellow colour, which is partly in small crystals, composed of soda and oxide of platina, in proportions not yet ascertained: these contain from 0·5 to 1 per cent. of chlorine. I consider this precipitate as a salt, which I shall call provisionally *platinate of soda*,

the symbol $\dot{N}a\ \ddot{P}l^2$. When heated to redness, it gives at first a quantity of water, then acid, and becomes black: the soda which it contains may then be separated by water. The black powder which remains appears to be a mixture of platina and oxide of platina, for it inflames a current of hydrogen passed over it, and gives, with muriatic acid, muriate of platina, and a black powder, insoluble in it, which becomes red in a detonating mixture of gases, and of a grayish white colour. With oxalic acid this substance acts as a mixture of oxide and of metal.

Acetic acid separates from the salt in question all the soda which it contains, and leaves oxide of platina, of an ochre yellow colour. A small portion of this oxide dissolves in the acid, without imparting any colour to it; from which it appears that oxide of platina dissolves with difficulty, or scarcely at all in acetic acid. It may be objected to this observation that muriate of platina is not precipitated by acetate of soda, and that the mixture of these salts in solution suffers no alteration, either by heat or solar light; but alcohol reduces the oxide which it contains to the spongy state, which does not happen unless it is combined with acetic acid, for the muriate is never reduced so quickly and so perfectly by alcohol.

Formic acid decomposes platinate of soda completely at a gentle heat, that is to say, all the oxide of platina is reduced, and a rapid disengagement of carbonic acid gas takes place. Eight grains of the salt, dried at 212°, gave with this acid five cubic inches of carbonic acid gas, consequently 1·05 grains of oxygen are combined with platina in eight grains of the salt.

The metal, reduced to the state of spongy platina, becomes instantly red hot when put upon printing paper slightly impregnated with alcohol.

Oxalic acid dissolves platinate of soda when heated, with the development of carbonic acid. A dark-coloured liquid is obtained, which, on cooling, at first becomes green, and afterwards of a deep magnificent blue colour: there are soon formed in it small needle-shaped crystals, of a deep copper red colour, and a metallic lustre, which are protacetate of platina $\dot{P}t\ \overline{O}$. These crystals, when heated, detonate: water separates, and carbonic acid is produced. The solution separated from the crystals is of a pale blue colour; diluted with water it becomes yellow, and by evaporation it becomes of a deep blue colour.

Dilute nitric acid readily dissolves platinate of soda; the solution is of a deep yellow colour. In a solution of nitrate of silver it gives a yellow precipitate, which is totally dissolved by nitric acid if the salt is free from chlorine; it is probably a nitroplatinate of silver.

If muriate of platina be mixed with a little cream of lime, then with a large quantity of lime water, and the filtered solution be exposed to the sun, it quickly becomes as turbid as milk, and after some hours it forms a flaky precipitate, which, after being boiled, is a yellowish white powder. According to Herschel *, this product is

* Sir John F. W. Herschel's observations on this substance will be found in Lond. and Edinb. Phil. Mag., vol. i. p. 59.—Edit.

platinate of lime; but according to my process for obtaining it, it is a compound of chloride of platina and platinate of lime ($\mathrm{Pt\ Ch}^2 + \underline{\dot{\mathrm{Ca}}}\ \ddot{\mathrm{Pt}}^3$).

If this compound be heated in a platina crucible to bright redness, it loses about 25 per cent., being water and a part of the oxygen combined with the platina. It becomes a deep violet-coloured powder, which becomes very hot when sprinkled with water, and when treated with dilute nitric acid, &c, is decomposed into muriate of lime, lime, and oxide of platina, of a deep violet colour.

This oxide is, I believe, the protoxide of platina ($\dot{\mathrm{Pt}}$), the base of the coppery oxalate already mentioned. This oxide does not dissolve in any oxyacid, but it combines by long digestion with oxalic acid. When treated with formic acid it is reduced to spongy platina, and evolves carbonic acid tumultuously, and in such quantity that its volume shows the quantity of oxygen contained in the protoxide. Eight grains of this oxide dried at 212°, being reduced by formic acid, gradually heated to ebullition, gave such a quantity of carbonic acid as showed its composition to be 92·204 metal, and 7·796 oxygen: according to Berzelius, the protoxide of platina contains 7·6 per cent. of oxygen. This difference probably arises from the circumstance of the oxide which I examined, and that once only, containing a small quantity of peroxide, or that the protoxide of Berzelius contained, as asserted by Liebig, some chlorine.—*Ann. de Chim. et de Phys.*, tom. liii. p. 204.

ON THE ROTARY MOTION OF CAMPHOR. BY M. CHARLES MATTEUCCI.

The phænomena exhibited by camphor, when put upon water, have been long known, and several philosophers have examined them; but if they are generally agreed as to the circumstances of these curious phænomena, this is not the case as to their cause. Thus, it has been said they were owing to the development of electricity, or to the solution of camphor, and lastly, to the evaporation of the camphor and water. It is easy to prove that it is not owing to the solution of the camphor in the water, for there are several substances which are much more soluble, but which do not turn when placed on water. Nothing indicates the development of electricity, and in this case it is not conceivable how it could produce the effect. The evaporation of water ought not to be considered in the explanation of the phænomena. It is therefore entirely to the evaporation of the camphor and its solution in the strata of water which surround it, that the cause of the motion must be attributed; and it is this opinion which I propose to develop and maintain.

Potassium is a substance which, when thrown on water, resembles camphor in the phænomena it produces: in this case it is to the disengagement of hydrogen and the vapour of water that the rapid movement must be attributed. It is even possible to imitate this rotation in a more simple manner; for this, it is sufficient to throw a small piece of red-hot charcoal upon water, or a very fine metallic

wire, suspended and previously heated. The motion, in this case, is produced simply by the vapour of water disengaged around the floating body. It is this hypothesis which very readily explains the suspension of rotation when a drop of oil is thrown on the surface of the water, or when it is covered with a plate of glass.

The following are the most convincing facts in favour of this explanation of the motion of camphor. I took rather a large piece of it, in order that when put upon water its motion might be very slow. I afterwards put the glass in which the experiment was made under the receiver of the air-pump and exhausted it. I then observed that the movements of the camphor, which were at first scarcely perceptible, become more rapid, and that they ceased when the action of the pump was stopped. On allowing the air to enter, the rotation occurred for some seconds, which was undoubtedly occasioned by the agitation produced by the reentering air. Lastly, and it is the most unexceptionable proof, I have observed these phænomena of rotation on water in all volatile bodies. I took raspings of cork and impregnated them with sulphuric æther: when placed upon water these small light bodies turned very rapidly. If it be wished to cause this rotation to continue for a long time, it is sufficient to immerse a wire in æther, and to make the other end touch the surface of the water; the æther descends as by a syphon, and the motion is continued.

It is, then, I think, proved that the rotation of volatile bodies is owing to the currents of their vapours. I will add a word respecting the well-known phænomenon produced by a stick of camphor immersed in water: I mean that of its being cut precisely according to the line which touches the exterior surface of the liquid. It is easy to prove, that of all the strata of water which are in contact with the camphor, it is in the upper that the solution is greatest. In fact, camphor dissolves in small quantity in water, but it is only at the surface that the dissolved camphor can evaporate: this water then dissolves a fresh portion, and so on repeatedly. When this solution is prevented, the phænomenon ceases to be produced. If a stick of camphor be placed in a concentrated solution of potash, and another in water, the latter is cut in two in three or four days, and the other is not at all attacked.—*Ann. de Chim. et de Phys.*, tom. liii. p. 216.

ON MARGARON, STEARON, AND OLEON.

M. Bussy prepared margaric acid by distilling suet, and purifying the product by pressure and crystallization in alcohol: it melted at 131° Fahrenheit. This acid was preferred to that obtained by saponification, because the margaric acid contained no stearic acid, and because it is more easily purified from the fluid products with which it is mixed.

A quantity of this acid was mixed with a quarter of its weight of lime, and distilled in a retort. First a quantity of water came over, and then a soft mass, from which there is obtained, by pressure, a matter similar to that which the suet furnishes. The last portions

of the acid undergo a more complete decomposition, for towards the end, the products are coloured and empyreumatic, and there remain in the retort a mixture of lime and carbonates and a small quantity of charcoal, which renders them black: 40 parts of margaric acid, treated in this manner, yielded 28 parts of a slightly yellowish solid, which soiled paper when pressed, and yielded 22 parts of dry matter, which melted at 165° Fahrenheit. It was repeatedly treated with alcohol of sp. gr. 0·837. After 11 digestions the fusing point of the last portion dissolved was 171°.

The substance thus obtained is of a pure white, brilliant and pearly when withdrawn from the alcohol from which it is precipitated. It fuses, as already mentioned, at 171°; crystallizes irregularly on cooling; and in appearance resembles margaric acid, or spermaceti. It is a nonconductor of electricity, and becomes strongly electrical by friction or pressure: when triturated in an agate mortar it often rises to the edges of the mortar, or to the pestle, and adheres to the paper used in removing it. If it be heated in a retort it boils, and distils without undergoing any notable change, and without leaving any residue. At a high temperature it burns with a very bright flame, and without smoke: paper or a cotton wick impregnated with it, burn in the same manner.

It dissolves in boiling alcohol of 0·837, but much less abundantly than margaric acid, requiring 50 times its weight. On cooling, the greater part of it separates, and water precipitates it. It dissolves in less than seven times its weight of alcohol of 0·817: the solution solidifies on cooling. Hot sulphuric æther dissolves more than $\frac{1}{4}$th of its weight, the greater part of which precipitates on cooling. Hot acetic æther dissolves it in large quantity, and on cooling a pearly mass is obtained: oil of turpentine produces a similar effect. It combines with camphor in all proportions: a strong and boiling solution of potash has no effect upon it. By sulphuric acid it is blackened, and completely decomposed with the disengagement of sulphurous acid. One part of it slightly heated with two parts of sulphuric acid, first became of a red colour, afterwards brown, then a deep black, and in a short time a coaly mass was obtained. Hot nitric acid acted but slightly upon it: by the action of dry chlorine gas, and at a low temperature, it was completely converted into a colourless, transparent, viscid liquid.

This substance, which M. Bussy calls *margaron,* bears some analogy to the paraffine of M. Reichenbach: it approaches it also in composition, as will presently be seen; but they differ in their melting point, margaron fusing at 171°, and paraffine at 111° Fahrenheit. Secondly, sulphuric acid does not act upon, but completely decomposes margaron. By analysis it yielded

Carbon	83·34
Hydrogen	13·51
Oxygen	3·15
	100·00

M. Bussy regards it as equivalent to margaric acid, less an atom of carbonic acid.

Stearic acid, treated in the same manner, yielded a substance which M. Bussy calls *stearon*, which resembles margaron in its external characters. When purified by crystallization in alcohol, it melts at 182° Fahrenheit, and it is less soluble than margaron in alcohol and æther. It is composed of

Carbon................	84·78
Hydrogen	13·77
Oxygen	1·45
	100·00

It appears to be equivalent to stearic acid deprived of carbonic acid.

Oleic acid, treated in the same manner as the two preceding, yielded also carbonate of lime as a residue, but the product distilled was fluid from the commencement of the operation. This fluid was not acid nor saponifiable, and appeared to be to oleic acid what margaron and stearon are to the preceding acids. The difficulty of obtaining pure oleic acid, and of separating *oleon* completely from other liquid products, which the distillation may have yielded, has hitherto prevented an analysis of it.—*Ann. de Chim. et de Phys.*, tom. liii. p. 398.

SUPPOSED COMPOUND OF HYDROGEN AND PLATINA. BY M. BOUSSINGAULT.

Berzelius (*Chimie*, tom. iii. p. 64.) mentions a supposed compound of hydrogen and platina, which may be thus prepared: Dissolve equal parts of iron and platina in aqua regia; the solution, deprived of its excess of acid, is to be precipitated by ammonia. The precipitate, when washed and dried, is to be reduced by hydrogen gas, in a glass tube heated to low redness: muriatic acid, muriate of ammonia, and the vapour of water are given out. The gas is to be passed until the tube cools. The residue in the tube is a mixture of platina and iron, which acts strongly as a pyrophorus: it requires some dexterity to put it into muriatic acid without its inflaming. The acid dissolves the iron with an abundant evolution of hydrogen gas; there remains a black powder, which is very heavy, and which only requires well washing.

This black powder, heated in an open vessel, inflames much below a red heat: sometimes it deflagrates, and the matter is thrown out in sparks; at other times the combustion takes place slowly, the powder burning like amadou. When heated in a close vessel, a little moisture appears on the cold part of it, and this circumstance gave rise to the opinion of the existence of hydrogen in it; but M. Boussingault considers it as unquestionably hygrometric moisture.

During combustion 311 parts of the powder increased in weight to 314, a circumstance which rendered the presence of iron extremely probable; but it is remarkable that the combustion occasions no change in the appearance of the powder; though it is no longer combustible, and 311 of the black powder, treated with boiling nitric acid, left 249 of finely divided platina: the acid contained only peroxide of iron, which amounted to $\frac{1}{5}$th of the powder. It is therefore very probable that the ignition is owing to the combustion of a part of the iron which is combined with the platina.

To prove that it contained no hydrogen, the black powder was mixed with peroxide of copper, and heated as in vegetable analyses; but the quantity of water obtained was such as to show that it could not contain $\frac{1}{1000}$th of its weight of hydrogen, and it is most probable that it did not contain any. On account of the difficulty of drying, the water was undoubtedly hygrometric. These experiments appear sufficient to prove that the supposed hydruret of platina is merely an alloy of iron and platina.

Descotils obtained from an alloy of platina and zinc, by means of dilute sulphuric acid, a black powder, which inflamed below a red heat. M. Boussingault found that it contained 31 per cent. of zinc, and its weight after combustion increased like that of the alloy of iron and platina. M. Boussingault proposes to examine, at a future opportunity, the black scales which Davy obtained by treating an alloy of platina and potassium with water, and which he considered as a hydruret of platina.—*Ann. de Chim. et de Phys.*, tom. liii. p. 441.

BORATES OF MAGNESIA. BY M. WÖHLER.

The boracite is a well known crystallized mineral, composed of boracic acid and magnesia. When solutions of borate of soda and borate of magnesia are mixed, no precipitation takes place until the mixture is heated, and then an abundant white precipitate is formed, which redissolves as the solution cools.

A solution in which the crystals had redissolved was exposed for several months to a temperature below 32° of Fahr. During this exposure, fine radiated acicular crystals were formed, some of them half an inch long: they were so slender that it was not possible to determine their crystalline form. These crystals are transparent, brilliant, hard and brittle, and perfectly insoluble in water, either cold or hot. Muriatic acid when hot decomposed these crystals, boracic acid being deposited on cooling; when heated they became opake and lost water. By analysis this salt yielded

Boracic acid	25·
Magnesia..............	16·67
Water	58·40
	100·07

The boracic acid and magnesia are in the same proportion as in boracite. In the case of this salt we have additional examples of the endless variety of symbols now inflicted upon the chemist. In the *Ann. de Chimie et de Phys.* it is represented by $\dot{Mg}\,\overset{\vdots\vdots}{B}{}^2 + 16\,\dot{\underline{H}}$, while in the *Journal de Chimie Médicale* we have $(2\,(Mg\,o) + \overline{B\,o^6} + 16\,H^2\,o)$, and yet both writers agree in considering it as similar to boracite combined with 16 atoms of water. Both differ from Berzelius.

From the solution which yielded these crystals there afterwards separated abundance of large brilliant, hard, transparent crystals in the form of oblique rhombic prisms.

This salt was found to be a double borate of soda and magnesia: when heated it swelled, and lost 52·5 per cent. of water of crystallization. The calcined residue redissolved in water, but so slowly that

it seemed at first to be insoluble: the solution was alkaline, and was not precipitated by ammonia. It has the characteristic property of becoming turbid when it is heated to about 160° Fahr. and depositing a great quantity of a white precipitate, which gradually redissolves as the solution cools. It is owing to the formation of this salt in the mixture of solutions of sulphate of magnesia and borax, that a precipitate is occasioned when it is heated. This precipitation is occasioned by the formation and precipitation of sub-borate of magnesia, while borate of soda and boracic acid remain in solution. On evaporating the liquor separated by filtration from the precipitate, boracic acid evaporates with the water, and a saline mass is obtained, from which alcohol separates a quantity of boracic acid. This sub-borate of magnesia is most readily obtained by heating a mixture of solution of borax and sulphate of magnesia.

Hydrate or carbonate of magnesia dissolves in a hot solution of boracic acid. The solution is alkaline: by evaporation it deposits a salt in crystalline grains, which is very soluble in water, although the solution goes on slowly. The solution is not precipitated at a high temperature, but when mixed with a solution of borax, it deposits a white precipitate if heated to about 160°. The precipitate disappears at common temperatures.

This borate of magnesia when heated to redness loses much water and boracic acid. The residue has the appearance of pumice-stone: water dissolves much pure boracic acid from it, and pure magnesia remains. It appears, then, that at high temperatures the affinity between magnesia and boracic acid is entirely destroyed.—*Ann. de Chim. et de Phys.*, tom. liii. p. 433.

ACTION OF TANNIN AND SOME OTHER SUBSTANCES ON THE ROOTS OF PLANTS. BY M. PAYEN.

It has been repeatedly stated by M. Silvestre, Jun., that trees soon died when their roots came into contact with the remains of the roots of oak trees cut down near them. This was supposed by some to be owing to the action of tannin, while others maintained that it was innocuous. M. Payen instituted direct experiments on this subject. In order to observe the influence of tannin and to appreciate its effects comparatively with those of other agents, M. Payen placed grains of wheat, rye, barley, oats, and maize, in contact with equal quantities of the following liquids, and all other circumstances were equal: 1st, aërated distilled water; 2nd, the same containing 0·01 of its weight of a saturated solution of carbonate of soda; 3rd, the same containing only 0·001 of its weight of the same saturated solution of carbonate of soda; 4th, a solution containing 0·001 of pure tannin; 5th, a solution containing 0·001 of sulphuric acid; 6th, distilled water saturated with lime.

In the distilled water, in the liquid containing 0·001 of solution of carbonate of soda, and in the solution of tannin, germination took place in the order stated; in the three other liquids, those containing 0·001 of acid, 0·001 of carbonate of soda, and saturated with lime, germination did not occur.

The distilled water soon became slightly acid. The development of the stalks, at first rather more rapid than in the solution of 0·001 of carbonate of soda, slackened comparatively with that which occurred in the last liquor: when this was neutralized by the acid excreted by vegetation, the original quantity of alkali was added. In bitter liquids, the white roots and green stalks were several centimetres long in a fortnight.

In the solution of 0·001 of tannin, all the radicles gradually acquired a brown tint, and were but slightly and imperfectly developed. The plumulæ continued whitish, but did not develop any green stalks; there was therefore a strongly marked obstacle to any further development in this liquid.

M. Payen concludes from the preceding and other experiments, that,

1st, Tannin, even in small quantity, acts deleteriously on the roots of certain plants:

2ndly, Acids in small proportion are hurtful to germination:

3rdly, Alkalies in small quantity are favourable to the progress of vegetation:

4thly, The saturation of the acidity developed during germination hastens its progress and favours the ulterior development.

These experiments account for one of the useful effects of lime, of vegetable ashes and calcareous marl, and for the unfavourable influence of alkalies used in too great quantity, or unequally distributed.—*Journal de Chimie Médicale,* Avril 1834.

DISCOVERY OF PLATINA IN FRANCE.

M. Villain has informed the Academy of Sciences of the discovery of a great mine of argentiferous galena: it is the mine of Melle, or Mello, situated in the departement de Deux Sevres. The mine contains two varieties of the ore, one with large facets, and the other very brilliant steel-grained ore. The former contains from 40 to 66 per cent. of lead: in cupelling the silver M. Villain obtained a blackish residue, which he suspected to be platina and iridium. Some of the samples of galena contained $\frac{22}{100,000}$ of its weight of platina, or 100 pounds of the lead should contain 1 ounce 7 gros and 46 grains of platina, and it is calculated that the daily product of platina will amount to 1 pound 4 ounces 2 gros and 28 grains. The steel-grained variety contains most platina.—*Journal de Chimie Médicale,* Feb. 1834.

PROFESSOR HAUSMANN ON MR. WHEWELL'S ACCOUNT OF HIS MINERALOGICAL WORKS.

We are requested by Professor Hausmann to insert the following remarks.

"Göttingen, July 5, 1834.

"The Report on the recent progress and present state of Mineralogy, by Mr. W. Whewell, contains the erroneous statement of my being a pupil of Mohs, and that I worked in the spirit and after the method of that master. To correct this error respecting my writings, and with regard to German mineralogical literature, I wish to state that I do not even personally know Mohs, though I

esteem him much. I studied at Brunswick under Knoch, and here at Göttingen under Blumenbach. Already in 1803, and therefore earlier than Mohs, I became a mineralogical writer, building my system on peculiar views belonging to no other school. I was the first who appeared as opponent to Werner; assisted in the spreading of Häuy's theory; and published my first mineralogical system in 1809, founded on chemical composition and external characters. I gave in 1813 a complete Handbuch on Mineralogy. Later I treated Crystallography on a method of my own, totally different from that of Mohs, though he was publishing at the same time; and in 1821 I completed my method in a quarto volume of 677 pages, entitled, 'Examinations on the Forms of Lifeless Nature;' and next, in the first volume of my new edition of Mineralogy, in 1828, which work I had the honour to present to the London Geological Society. Had Mr. W. looked into this book, he might have convinced himself of his erroneous assertion; and as it is not indifferent to me how England judges of me, I should be greatly obliged if through your Journal his statement could be rectified. HAUSMANN."

IMPROVEMENT IN PROFESSOR HENSLOW'S CLINOMETER. BY J. H. PRATT, ESQ.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I have lately had occasion to purchase one of the Clinometers sold by Messrs. Watkins and Hill, Charing Cross, and invented, I believe, by Professor Henslow; but being struck with the want of accuracy, or rather the liability of error, in the determination of the dip of a stratum by its means, I have been induced to make a slight alteration in the construction. My own I have had altered, and find it answers very well, and the object of my troubling you with this communication is to suggest the same to any person that may have experienced the same inconvenience with myself. The only alteration is to have the spirit-level on the outside of the lid of the box, instead of the inside of the bottom of the box. With this construction it is necessary merely to place the box flat on the bed of the stratum and elevate the lid till the spirit-level shows that the lid is horizontal: the brazen arc will then show the dip.

I remain, yours, &c.

Finsbury Circus, June 12, 1834. J. H. PRATT.

P.S. The spirit-level is so imbedded in the wood that there is no fear of its being broken.

PRIMARY GEOLOGY.

We are informed that Dr. Boase's Treatise on Primary Geology, just published, contains, in addition to copious practical details concerning the primary rocks, a full exposition of his objections to the prevailing Plutonic Theory. This subject, it may be remembered, was appointed by the British Association for the Advancement of Science, at Cambridge, to be discussed at their next meeting at Edinburgh.

Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London and by Mr. VEALL at Boston.

Days of Month. 1834.	Barometer. London. Max.	Barometer. London. Min.	Barometer. Boston $8\frac{1}{2}$ A.M.	Thermometer. London. Max.	Thermometer. London. Min.	Thermometer. Bost. $8\frac{1}{2}$ A.M.	Wind. Lond.	Wind. Bost.	Rain. Lond.	Rain. Bost.
June 1	30·329	30·281	29·83	81	45	60	s.	calm	...	...
2	30·212	30·103	29·63	85	49	66·5	s.	NW.	...	...
3	30·050	30·024	29·40	74	45	65	w.	calm	0·03	...
4	30·011	29·724	29·35	70	49	63·5	sw.	w.	·20	...
5	29·973	29·681	29·22	65	39	55	NW.	N.	...	·26
6	30·156	30·091	29·58	70	42	61	N.	w.	...	·06
● 7	30·123	30·061	29·65	78	45	63	SE.	calm	...	...
8	29·879	29·830	29·43	78	40	64	N.	calm	...	...
9	29·778	29·730	29·22	78	51	66·5	s.	w.	...	...
10	29·697	29·654	29·11	73	43	63	sw.	w.	·05	...
11	29·746	29·699	29·14	64	42	61	s.	w.	·22	...
12	29·772	29·496	29·25	68	50	59	s.	w.	·60	·09
13	29·875	29·740	29·15	69	55	59	sw.	NW.	·01	·12
☽ 14	29·928	29·774	29·32	75	55	57·5	w.	w.	·02	·04
15	29·885	29·753	29·26	74	56	66	w.	w.	...	·10
16	29·674	29·644	29·06	68	51	64	w.	w	·01	...
17	29·965	29·736	29·12	67	47	58·5	w.	w.	·04	·24
18	30·063	30·020	29·46	68	59	63	w.	calm	...	...
19	30·121	30·096	29·47	76	50	66	sw.	calm	...	·04
20	30·105	29·903	29·45	89	57	67	s.	w.	...	...
○ 21	29·860	29·800	29·10	91	58	78·5	s.	w.	·38	...
22	30·099	29·904	29·18	73	47	67	w.	w.	...	·35
23	30·281	30·247	29·63	76	43	62	sw.	w.	...	...
24	30·327	30·299	29·70	73	56	63	s.	w.	...	...
25	30·329	30·261	29·66	77	57	66	s.	w.	...	...
26	30·222	30·077	29·47	73	54	69	w.	w.	·07	...
27	30·284	30·096	29·50	67	41	60	NE.	w.	...	·06
28	30·244	30·157	29·70	74	49	62·5	SE.	w.	...	...
☾ 29	30·339	30·176	29·70	72	46	63	E.	calm		...
30	30·399	30·308	29·86	74	46	64	E.	calm	...	...
	30·399	29·469	29·42	91	39	6·34			1·63	1·36

Remarks.

London.—June 1, 2. Very hot and dry. 3. Slight rain. 4. Cloudy: rain. 5. Cloudy: fine. 6—9. Very fine. 10. Dark clouds. 11. Rain. 12. Heavy rain. 13. Overcast and windy. 14—16. Very fine. 17. Showery. 18. Cloudy and cool. 19. Fine. 20. Very hot. 21. Excessively hot and dry: thermometer 91° in shade: lightning and heavy rain at night. 22. Cloudy. 23, 24. Hot and dry. 25. Hazy. 26, 27. Fine, with clouds. 28. Fine. 29. Dry haze. 30 Fine. —A moderate quantity of rain has fallen in this month, but still below the average, and not in proportion to the high temperature, nor, consequently, equal to the demands of many kinds of vegetation. The latter, however, would have suffered much more had not the supply, limited as it was, been at such intervals throughout the month as to leave them exposed to no protracted period of drought.

Boston.—June 1. Fine. 2. Fine: thermometer 3 P.M. 83°. 3. Fine. 4. Fine: rain P.M. 5. Rain: rain early A.M. 6. Cloudy. 7—9. Fine. 10. Cloudy. 11, 12. Fine: rain P.M. 13. Fine. 14. Fine: rain early A.M.: rain with thunder and lightning P.M. 15. Fine. 16. Cloudy: stormy with rain P.M. 17. Stormy. 18. Fine: rain P.M. 19, 20. Cloudy. 21. Fine: thermometer at noon 86°; 2 P.M, 87°·5; half-past 3, 81°·5: rain P.M. 22. Cloudy. 23, 24. Fine. 25, 26. Cloudy. 27. Cloudy: rain early A.M. 28. Fine. 29. Cloudy. 30. Fine.

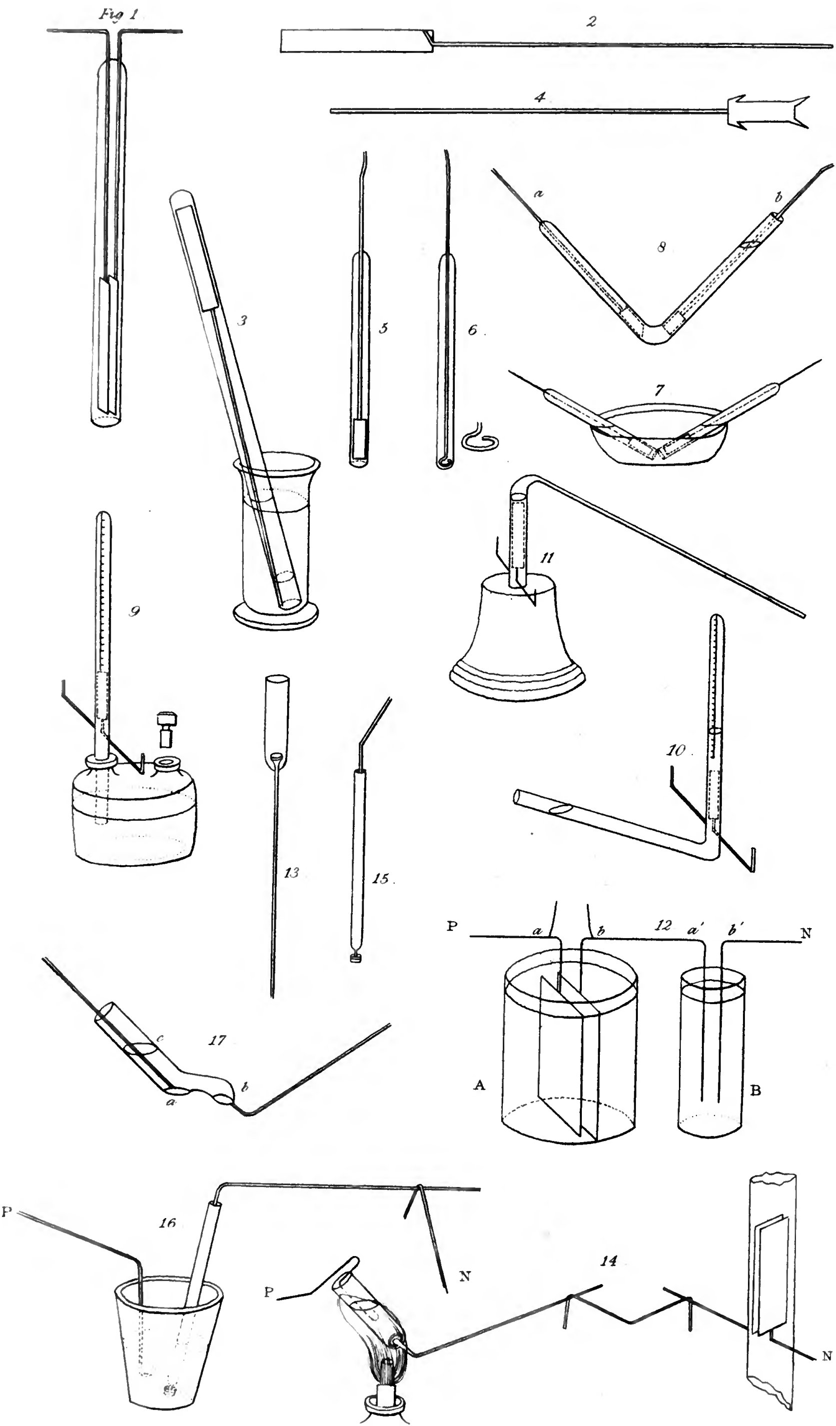


Fig 1
2
4
3
5
6
a
b
8
7
9
11
10
13
15
P
a
b
12
a'
b'
N
A
B
c
17
a
b
P
16
N
P
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THE

LONDON AND EDINBURGH

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

SEPTEMBER 1834.

XXV. *Experimental Researches in Electricity.—Seventh Series. By* MICHAEL FARADAY, *D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c.**

§. 11. *On Electro-chemical Decomposition, continued.* ¶ iv. *On some general conditions of Electro-decomposition.* ¶ v. *On a new Measurer of Volta-electricity.* ¶ vi. *On the primitive or secondary character of bodies evolved in Electro-decomposition.* ¶ vii. *On the definite nature and extent of Electro-chemical Decompositions.* §. 13. *On the absolute quantity of Electricity associated with the particles or atoms of Matter.*

Preliminary.

661. THE theory which I believe to be a true expression of the facts of electro-chemical decomposition, and which I have therefore detailed in a former series of these Researches†, is so much at variance with those previously advanced, that I find the greatest difficulty in stating results, as I think, correctly, whilst limited to the use of terms which are current with a certain accepted meaning. Of this kind is the term pole, with its prefixes of positive and negative, and the attached ideas of attraction and repulsion. The general phraseology is that the positive pole *attracts* oxygen, acids,

* From the Philosophical Transactions for 1834, Part I. p. 77. This paper was received by the Royal Society January 9th, and read January 23rd, February 6th and 13th, 1834.

† A notice of the Researches here referred to has been given in the Lond. and Edinb. Phil. Mag. vol. iii. p. 460.—EDIT.

&c., or more cautiously, that it *determines* their evolution upon the surface; and that the negative pole acts in an equal manner upon hydrogen, combustibles, metals, and bases. According to my view, the determining force is *not* at the poles, but *within* the decomposing body; and the oxygen and acids are rendered at the *negative* extremity of that body, whilst hydrogen, metals, &c., are evolved at the *positive* extremity (518. 524.*).

662. To avoid, therefore, confusion and circumlocution, and for the sake of greater precision of expression than I can otherwise obtain, I have deliberately considered the subject with two friends, and with their assistance and concurrence in framing them, I purpose henceforward using certain other terms, which I will now define. The poles, as they are usually called, are only the doors or ways by which the electric current passes into and out of the decomposing body (556.); and they of course, when in contact with that body, are the limits of its extent in the direction of the current. The term has been generally applied to the metal surfaces in contact with the decomposing substance; but whether philosophers generally would also apply it to the surfaces of air (465. 471.) and water (493.), against which I have effected electro-chemical decomposition, is subject to doubt. In place of the term pole, I propose using that of *Electrode*†, and I mean thereby that substance, or rather surface, whether of air, water, metal, or any other body, which bounds the extent of the decomposing matter in the direction of the electric current.

663. The surfaces at which, according to the common phraseology, the electric current enters and leaves a decomposing body, are most important places of action, and require to be distinguished apart from the poles, with which they are mostly, and the electrodes, with which they are always, in contact. Wishing for a natural standard of electric direction to which I might refer these, expressive of their difference and at the same time free from all theory, I have thought it might be found in the earth. If the magnetism of the earth be due to electric currents passing round it, the latter must be in a constant direction, which, according to present usage of speech, would be from east to west, or, which will strengthen this help to the memory, that in which the sun appears to move. If in any case of electro-decomposition we consider the decomposing body as placed so that the current passing through it shall be in the same direction, and parallel to that supposed to exist in the earth, then the surfaces at which the

* These numbers, and others referred to in these Researches from 450. to 563., both inclusive, belong to the Fifth Series, noticed in our third volume, as already stated.—Edit.

† ἤλεκτρον, and ὁδὸς *a way*.

electricity is passing into and out of the substance would have an invariable reference, and exhibit constantly the same relations of powers. Upon this notion we purpose calling that towards the east the *anode**, and that towards the west the *cathode*†; and whatever changes may take place in our views of the nature of electricity and electrical action, as they must affect the natural standard referred to in the same direction, and to an equal amount with any decomposing substances to which these terms may at any time be applied, there seems no reason to expect that they will lead to confusion, or tend in any way to support false views. The *anode* is therefore that surface at which the electric current, according to our present expression, enters: it is the negative extremity of the decomposing body; is where oxygen, chlorine, acids, &c., are evolved; and is against or opposite the positive electrode. The *cathode* is that surface at which the current leaves the decomposing body, and is its positive extremity; the combustible bodies, metals, alkalies, and bases, are evolved there, and it is in contact with the negative electrode.

664. I shall have occasion in these Researches, also, to class bodies together according to certain relations derived from their electrical actions (822.); and wishing to express those relations without at the same time involving the expression of any hypothetical views, I intend using the following names and terms. Many bodies are decomposed directly by the electric current, their elements being set free; these I propose to call *electrolytes*‡. Water, therefore, is an electrolyte. The bodies which, like nitric or sulphuric acids, are decomposed in a secondary manner (752. 757.), are not included under this term. Then for *electro-chemically decomposed*, I shall often use the term *electrolyzed*, derived in the same way, and implying that the body spoken of is separated into its components under the influence of electricity: it is analogous in its sense and sound to *analyze*, which is derived in a similar manner. The term *electrolytical* will be understood at once. Muriatic acid is electrolytical, boracic acid is not.

665. Finally, I require a term to express those bodies which can pass to the *electrodes*, or, as they are usually called, the poles. Substances are frequently spoken of as being *electro-negative*, or *electro-positive*, according as they go under the supposed influence of a direct attraction to the positive or negative pole. But these terms are much too significant for the use to which I should have to put them; for though the

* *ἀνα upwards, ὁδος a way*; the way which the sun rises.
† *κατα downwards, ὁδος a way*; the way which the sun sets.
‡ *ἤλεκτρον*, and *λυω solvo*. N. Electrolyte, V. Electrolyze.

meanings are perhaps right, they are only hypothetical, and may be wrong; and then, through a very imperceptible, but still very dangerous, because continual, influence, they do great injury to science, by contracting and limiting the habitual views of those engaged in pursuing it. I propose to distinguish these bodies by calling those *anions** which go to the *anode* of the decomposing body; and those passing to the *cathode*, *cations*†; and when I have occasion to speak of these together, I shall call them *ions*. Thus, the chloride of lead is an *electrolyte*, and when *electrolyzed* evolves the two *ions*, chlorine and lead, the former being an *anion*, and the latter a *cation*.

666. These terms being once well defined, will, I hope, in their use enable me to avoid much periphrasis and ambiguity of expression. I do not mean to press them into service more frequently than will be required, for I am fully aware that names are one thing and science another‡.

667. It will be well understood that I am giving no opinion respecting the nature of the electric current now, beyond what I have done on a former occasion (283.§ 517.); and that though I speak of the current as proceeding from the parts which are positive to those which are negative (663.), it is merely in accordance with the conventional, though in some degree tacit, agreement entered into by scientific men, that they may have a constant, certain, and definite means of referring to the direction of the forces of that current.

¶ iv. *On some general Conditions of Electro-chemical Decomposition.*

669. From the period when electro-chemical decomposition was first effected to the present time, it has been a remark, that those elements which, in the ordinary phænomena of chemical affinity, were the most directly opposed to each other, and combined with the greatest attractive force, were those which were the most readily evolved at the opposite extremities of the decomposing bodies (549.).

670. If this result was evident when water was supposed to be essential to, and was present, in almost every case of such decomposition (472.), it is far more evident now that it has been shown and proved that water is not necessarily con-

* *ἀνιον that which goes up.* (Neuter participle.)

† *κατιον that which goes down.*

‡ Since this paper was read, I have changed some of the terms which were first proposed, that I might employ only such as were at the same time simple in their nature, clear in their reference, and free from hypothesis.

§ See Lond. and Edinb. Phil. Mag. vol. iii. p. 166.—Edit.

cerned in the phænomena (474.), and that other bodies much surpass it in some of the effects supposed to be peculiar to that substance.

671. Water, from its constitution and the nature of its elements, and from its frequent presence in cases of electrolytic action, has hitherto stood foremost in this respect. Though a compound formed by very powerful affinity, it yields up its elements under the influence of a very feeble electric current; and it is doubtful whether a case of electrolyzation can occur, where, being present, it is not resolved into its first principles.

672. The various oxides, chlorides, iodides, and salts (402.), which I have shown are decomposable by the electric current when in the liquid state, under the same general law with water, illustrate in an equally striking manner the activity, in such decompositions, of elements directly and powerfully opposed to each other by their chemical relations.

673. On the other hand, bodies dependent on weak affinities very rarely give way. Take, for instance, glasses: many of those formed of silica, lime, alkali, and oxide of lead, may be considered as little more than solutions of substances one in another*. If bottle-glass be fused, and subjected to the voltaic pile, it does not appear to be at all decomposed (408.). If flint-glass, which contains substances more directly opposed, be operated upon, it suffers some decomposition; and if borate of lead glass, which is a definite chemical compound, be experimented with, it readily yields up its elements (408.*).

674. But the result which is found to be so striking in the instances quoted is not at all borne out by reference to other cases where a similar consequence might have been expected. It may be said, that my own theory of electro-chemical decomposition would lead to the expectation that all compound bodies should give way under the influence of the electric current with a facility proportionate to the strength of the affinity by which their elements, either proximate or ultimate, are combined. I am not sure that that follows as a consequence of the theory; but if the objection be supposed one presented by facts, I have no doubt it will be removed when we obtain a more intimate acquaintance with, and precise idea of, the nature of chemical affinity and the mode of action of an electric current over it (518. 524.)): besides which, it is just as directly opposed to any other theory of electro-chemical

* Philosophical Transactions, 1830, p. 49.

† These numbers, and the others referred to from 380. to 449., both inclusive, belong to the Fourth Series of these Researches, noticed in Lond. and Edinb. Phil. Mag. vol. iii. p. 449.—Edit.

decomposition as the one I have propounded; for if it be admitted, as is generally the case, that the more directly bodies are opposed to each other in their attractive forces, the more powerfully do they combine, then the objection applies with equal force to any of the theories of electrolyzation which have been considered, and is an addition to those which I have taken against them.

675. Amongst powerful compounds which are not decomposed, boracic acid stands prominent (408.). Then again, the iodide of sulphur, and the chlorides of sulphur, phosphorus, and carbon, are not decomposable under common circumstances, though their elements are of a nature which would lead to a contrary expectation. Chloride of antimony (402. 690.), the hydro-carbons, acetic acid, ammonia, and many other bodies undecomposable by the voltaic pile, would seem to be formed by an affinity sufficiently strong to indicate that the elements were so far contrasted in their nature as to sanction the expectation that the pile would separate them, especially as in some cases of mere solution (530. 544.), where the affinity must by comparison be very weak, separation takes place*.

676. It must not be forgotten, however, that much of this difficulty, and perhaps the whole, may depend upon the absence of conducting power, which, preventing the transmission of the current, prevents of course the effects due to it. All known compounds being non-conductors when solid, but conductors when liquid, are decomposed, with *perhaps* the single exception at present known of periodide of mercury (679. 691.); and even water itself, which so easily yields up its elements when the current passes, if rendered quite pure, scarcely suffers change, because it then becomes a very bad conductor.

677. If it should hereafter be proved that the want of decomposition in those cases where, from chemical considerations, it might be so strongly expected (669. 674. 672.), is due to the absence or deficiency of conducting power, it would also be proved, at the same time, that decomposition *depends* upon conduction, and not the latter upon the former (413.); and in water this seems to be very nearly decided. On the other hand, the conclusion is almost irresistible, that in electrolytes the power of transmitting the electricity across the substance is dependent upon their capability of suffering decomposition; taking place only whilst they are decomposing,

* With regard to solution, I have met with some reasons for supposing that it will probably disappear as a cause of transference, and intend resuming the consideration at a convenient opportunity.

and being proportionate to the quantity of elements separated (821.). I may not, however, stop to discuss this point experimentally at present.

678. When a compound contains such elements as are known to pass towards the opposite extremities of the voltaic pile, still the proportions in which they are present appear to be intimately connected with capability in the compound of suffering or resisting decomposition. Thus, the protochloride of tin readily conducts, and is decomposed (402.), but the perchloride neither conducts nor is decomposed (406.). The protiodide of tin is decomposed when fluid (402.); the periodide is not (405.). The periodide of mercury when fused is not decomposed (691.), even though it does conduct. I was unable to contrast it with the protiodide, the latter being converted into mercury and periodide by heat.

679. These important differences induced me to look more closely to certain binary compounds, with a view of ascertaining whether a *law* regulating the *decomposability* according to some *relation of the proportionals or equivalents* of the elements, could be discovered. The proto compounds only, amongst those just referred to, were decomposable; and on referring to the substances quoted to illustrate the force and generality of the law of conduction and decomposition which I discovered (402.), it will be found that all the oxides, chlorides, and iodides subject to it, except the chloride of antimony and the periodide of mercury, (to which may now perhaps be added corrosive sublimate,) are also decomposable, whilst many per compounds of the same elements, not subject to the law, were not so (405. 406.).

680. The substances which appeared to form the strongest exceptions to this general result were such bodies as the sulphuric, phosphoric, nitric, arsenic, and other acids.

681. On experimenting with sulphuric acid, I found no reason to believe that it was by itself a conductor of, or decomposable by, electricity, although I had previously been of that opinion (552.). When very strong it is a much worse conductor than if diluted*. If then subjected to the action of a powerful battery, oxygen appears at the *anode*, or positive electrode, although much is absorbed (728.), and hydrogen and sulphur appear at the *cathode*, or negative electrode. Now the hydrogen has with me always been pure, not sulphuretted, and has been deficient in proportion to the sulphur present, so that it is evident that when decomposition occurred water must have been decomposed. I endeavoured to make the experiment with anhydrous sulphuric acid. It ap-

* De la Rive.

peared to me that in that state, when fused, sulphuric acid was not a conductor, nor decomposed; but I had not enough of the dry acid in my possession to allow me to decide the point satisfactorily. My belief is, that when sulphur appears by the action of the pile on sulphuric acid, it is the result of a secondary action, and that the acid itself is not electrolyzable (757.).

682. Phosphoric acid is, I believe, also in the same condition; but I have found it impossible to decide the point, because of the difficulty of operating on fused anhydrous phosphoric acid. Phosphoric acid which has once obtained water cannot be deprived of it by heat alone. When heated, the hydrated acid volatilizes. Upon subjecting phosphoric acid, fused upon the ring end of a wire (401.), to the action of the voltaic apparatus, it conducted, and was decomposed; but gas, which I believe to be hydrogen, was always evolved at the negative electrode, and the wire was not affected as would have happened had phosphorus been separated. Gas was also evolved at the positive electrode. From all the facts, I conclude it was the water and not the acid which was decomposed.

683. *Arsenic Acid.*—This substance conducted, and was decomposed; but it contained water, and I was unable at the time to press the investigation so as to ascertain whether a fusible anhydrous arsenic acid could be obtained. It forms, therefore, at present no exception to the general result.

684. Nitrous acid, obtained by distilling nitrate of lead, and keeping it in contact with strong sulphuric acid, was found to conduct and decompose slowly. But on examination there were strong reasons for believing that water was present, and that the decomposition and conduction depended upon it. I endeavoured to prepare a perfectly anhydrous portion, but could not spare the time required to procure an unexceptionable result.

685. Nitric acid is a substance which I believe is not decomposed directly by the electric current. As I want the facts in illustration of the distinction existing between primary and secondary decomposition, I will merely refer to them in this place (752.).

686. That these mineral acids should confer facility of conduction and decomposition on water, is no proof that they are competent to favour and suffer these actions in themselves. Boracic acid does the same thing, though not decomposable. M. De la Rive has pointed out that chlorine has this power also; but being to us an elementary substance, it cannot be due to its capability of suffering decomposition.

687. *Chloride of sulphur* does not conduct, nor is it decom-

posed. It consists of single proportionals of its elements, but is not on that account an exception to the rule (679.), which does not affirm that *all* compounds of single proportionals of elements are decomposable, but that such as are decomposable are so constituted.

688. *Protochloride of phosphorus* does not conduct nor become decomposed.

689. *Protochloride of carbon* does not conduct nor suffer decomposition. In association with this substance, I submitted the *hydro-chloride of carbon* from olefiant gas and chlorine to the action of the electric current; but it also refused to conduct or yield up its elements.

690. With regard to the exceptions (679.), upon closer examination, some of them disappear. Chloride of antimony (a compound of one proportional of antimony and one and a half of chlorine) of recent preparation was put into a tube (Plate I. fig. 13.) (789.), and submitted when fused to the action of the current, the positive electrode being of plumbago. No electricity passed, and no appearance of decomposition was visible at first; but when the positive and negative electrodes were brought very near each other in the chloride, then a feeble action occurred and a feeble current passed. The effect altogether was so small (although quite amenable to the law before given), and so unlike the decomposition and conduction occurring in all the other cases, that I attribute it to the presence of a minute quantity of water, (for which this and many other chlorides have strong attractions, producing hydrated chlorides,) or perhaps of a true protochloride consisting of single proportionals (695. 796.).

691. *Periodide of mercury* being examined in the same manner, was found most distinctly to insulate whilst solid, but conduct when fluid, according to the law of *liquido-conduction* (402.); but there was no appearance of decomposition. No iodine appeared at the *anode*, nor mercury or other substance at the *cathode*. The case is, therefore, no exception to the rule, that only compounds of single proportionals are decomposable; but it is an exception, and I think the only one, to the statement, that all bodies subject to the law of liquido-conduction are decomposable. I incline, however, to believe, that a portion of protiodide of mercury is retained dissolved in the periodide, and that to its slow decomposition the feeble conducting power is due. Periodide would be formed, as a secondary result, at the *anode*; and the mercury at the *cathode* would also form, as a secondary result, protiodide. Both these bodies would mingle with the fluid mass, and thus no final separation appear, notwithstanding the continued decomposition.

692. When *perchloride of mercury* was subjected to the voltaic current, it did not conduct in the solid state, but it did conduct when fluid. I think, also, that in the latter case it was decomposed; but there are many interfering circumstances which require examination before a positive conclusion can be drawn.

693. When the ordinary protoxide of antimony is subjected to the voltaic current in a fused state, it also is decomposed, although the effect from other causes soon ceases (402. 801.). This oxide consists of one proportional of antimony and one and a half of oxygen, and is therefore an exception to the general law assumed. But in working with this oxide and the chloride, I observed facts which lead me to doubt whether the compounds usually called the protoxide and the protochloride do not often contain other compounds, consisting of single proportions, which are the true proto compounds, and which, in the case of the oxide, might give rise to the decomposition above described.

694. The ordinary sulphuret of antimony is considered as being the compound with the smallest quantity of sulphur, and analogous in its proportions to the ordinary protoxide. But I find that if it be fused with metallic antimony, a new sulphuret is formed, containing much more of the metal than the former, and separating distinctly, when fused, both from the pure metal on the one hand, and the ordinary gray sulphuret on the other. In some rough experiments, the metal thus taken up by the ordinary sulphuret of antimony was equal to half the proportion of that previously in the sulphuret, in which case the new sulphuret would consist of *single* proportionals.

695. When this new sulphuret was dissolved in muriatic acid, although a little antimony separated, yet it appeared to me that a true protochloride, consisting of *single* proportionals, was formed, and from that, by alkalies, &c., a true protoxide, consisting also of *single* proportionals was obtainable. But I could not stop to ascertain this matter strictly by analysis.

696. I believe, however, that there is such an oxide; that it is often present in variable proportions in what is commonly called protoxide, throwing uncertainty upon the results of its analysis, and causing the electrolytic decomposition above described.

697. Upon the whole, it appears probable that all those binary compounds of elementary bodies which are capable of being electrolyzed when fluid, but not whilst solid, according to the law of liquido-conduction (394.), consist of single proportionals of their elementary principles; and it may be be-

cause of their departure from this simplicity of composition, that boracic acid, ammonia, perchlorides, periodides, and many other direct compounds of elements, are indecomposable.

698. With regard to salts and combinations of compound bodies, the same simple relation does not appear to hold good. I could not decide this by bisulphates of the alkalies, for as long as the second proportion of acid remained, water was retained with it. The fused salt, therefore, conducted, and was decomposed; but hydrogen always appeared at the negative electrode.

699. A biphosphate of soda was prepared by heating, and ultimately fusing, the ammonia-phosphate of soda. In this case the fused bisalt conducted, and was decomposed; but a little gas appeared at the negative electrode, and though I believe the salt itself was electrolyzed, I am not quite satisfied that water was entirely absent.

700. Then a biborate of soda was prepared; and this, I think, is an unobjectionable case. The salt, when fused, conducted, and was decomposed, and gas appeared at both electrodes: even when the boracic acid was increased to three proportionals the same effect took place.

701. Hence this class of compound combinations does not seem to be subject to the same simple law as the former class of binary combinations. Whether we may find reason to consider them as mere solutions of the compound of single proportionals in the excess of acid, is a matter which, with some apparent exceptions occurring amongst the sulphurets, must be left for decision by future examination.

702. In any investigation of these points, great care must be taken to exclude water; for if present, secondary effects are so frequently produced as often seemingly to indicate an electro-decomposition of substances, when no true result of the kind has occurred (742. &c.).

703. It is evident that all the cases in which decomposition *does not occur may* depend upon the want of conduction (677. 413.); but that does not at all lessen the interest excited by seeing the great difference of effect due to a change, not in the nature of the elements, but merely in their proportions, especially in any attempt which may be made to elucidate and expound the beautiful theory put forth by Sir Humphry Davy*, and illustrated by Berzelius and other eminent philosophers, that ordinary chemical affinity is a mere result of the electrical attractions of the particles of matter.

* Philosophical Transactions, 1807, pp. 32, 39; also 1826, pp, 387, 389. [or Phil. Mag. first series, vol. xxviii. pp. 114, 220; also Phil. Mag. and Annals, N.S., vol. i. p. 31–199.—Edit.]

¶ v. *On a new Measurer of Volta-electricity.*

704. I have already said, when engaged in reducing common and voltaic electricity to one standard of measurement (377*.), and again when introducing my theory of electro-chemical decomposition (504. 505. 510.), that the chemical decomposing action of a current *is constant for a constant quantity of electricity*, notwithstanding the greatest variations in its sources, in its intensity, in the size of the *electrodes* used, in the nature of the conductors (or non-conductors (307.†)) through which it is passed, or in other circumstances. The conclusive proofs of the truth of these statements shall be given almost immediately (783. &c.).

705. I endeavoured upon this law to construct an instrument which should measure out the electricity passing through it, and which, being interposed in the course of the current used in any particular experiment, should serve at pleasure, either as a *comparative standard* of effect, or as a *positive measurer* of this subtile agent.

706. There is no substance better fitted, under ordinary circumstances, to be the indicating body in such an instrument than water; for it is decomposed with facility when rendered a better conductor by the addition of acids or salts; its elements may in numerous cases be obtained and collected without any embarrassment from secondary action, and, being gaseous, they are in the best physical condition for separation and measurement. Water, therefore, acidulated by sulphuric acid, is the substance I shall generally refer to, although it may become expedient in peculiar cases or forms of experiment to use other bodies (843.).

707. The first precaution needful in the construction of the instrument was to avoid the recombination of the evolved gases, an effect which the positive electrode has been found so capable of producing (571.‡). For this purpose various forms of decomposing apparatus were used. The first consisted of straight tubes, each containing a plate and wire of platina soldered together by gold, and fixed hermetically in the glass at the closed extremity of the tube (Plate I. fig. 5.) The tubes were about eight inches long, 0·7 of an inch in diameter, and graduated. The platina plates were about an inch long, as wide as the tubes would permit, and adjusted as near

* See Lond. and Edinb. Phil. Mag., vol. iii. p. 362.—Edit.

† Ibid. p. 171.

‡ This and every other number referred to in these Researches, from 564. to 660., belong to the Sixth Series, noticed in Lond. and Edinb. Phil. Mag., vol. iv. p. 291.—Edit.

to the mouths of the tubes as was consistent with the safe collection of the gases evolved. In certain cases, where it was required to evolve the elements upon as small a surface as possible, the metallic extremity, instead of being a plate, consisted of the wire bent into the form of a ring (fig. 6.). When these tubes were used as measurers, they were filled with the dilute sulphuric acid, and inverted in a basin of the same liquid (fig. 7.), being placed in an inclined position, with their mouths near to each other, that as little decomposing matter should intervene as possible; and also, in such a direction that the platina plates should be in vertical planes (720.).

708. Another form of apparatus was that delineated (fig. 8.). The tube is bent in the middle; one end is closed; in that end is fixed a wire and plate, *a*, proceeding so far downwards, that, when in the position figured, it shall be as near to the angle as possible, consistently with the collection, at the closed extremity of the tube, of all the gas evolved against it. The plane of this plate is also perpendicular (720.). The other metallic termination, *b*, is introduced at the time decomposition is to be effected, being brought as near the angle as possible, without causing any gas to pass from it towards the closed end of the instrument. The gas evolved against it is allowed to escape.

709. The third form of apparatus contains both electrodes in the same tube; the transmission, therefore, of the electricity, and the consequent decomposition, is far more rapid than in the separate tubes. The resulting gas is the sum of the portions evolved at the two electrodes, and the instrument is better adapted than either of the former as a measurer of the quantity of voltaic electricity transmitted in ordinary cases. It consists of a straight tube (fig. 9.) closed at the upper extremity, and graduated, through the sides of which pass the platina wires (being fused into the glass), which are connected with two plates within. The tube is fitted by grinding into one mouth of a double-necked bottle. If the latter be one half or two thirds full of the dilute sulphuric acid, it will, upon inclination of the whole, flow into the tube and fill it. When an electric current is passed through the instrument, the gases evolved against the plates collect in the upper portion of the tube, and are not subject to the recombining power of the platina.

710. Another form of the instrument is given at fig. 10.

711. A fifth form is delineated (fig. 11.). This I have found exceedingly useful in experiments continued in succession for days together, and where large quantities of indicating gas were to be collected. It is fixed on a weighted foot,

and has the form of a small retort containing the two electrodes: the neck is narrow, and sufficiently long to deliver gas issuing from it into a jar placed in a small pneumatic trough. The electrode chamber, sealed hermetically at the part held in the stand, is five inches in length, and 0·6 of an inch in diameter; the neck about nine inches in length, and 0·4 of an inch in diameter internally. The figure will fully indicate the construction.

712. It can hardly be requisite to remark, that in the arrangement of any of these forms of apparatus, they, and the wires connecting them with the substance, which is collaterally subjected to the action of the same electric current, should be so far insulated as to ensure a certainty that all the electricity which passes through the one shall also be transmitted through the other.

713. Next to the precaution of collecting the gases, if mingled, out of contact with the platinum, was the necessity of testing the law of a *definite electrolytic* action, upon water at least, under all varieties of condition; that, with a conviction of its certainty, might also be obtained a knowledge of those interfering circumstances which would require to be practically guarded against.

714. The first point investigated was the influence or indifference of extensive variations in the size of the electrodes, for which purpose instruments like those last described (709. 710. 711.) were used. One of these had plates 0·7 of an inch wide, and nearly four inches long; another had plates only 0·5 of an inch wide, and 0·8 of an inch long; a third had wires 0·02 of an inch in diameter, and three inches long; and a fourth similar wires only half an inch in length. Yet when these were filled with dilute sulphuric acid, and, being placed in succession, had one common current of electricity passed through them, very nearly the same quantity of gas was evolved in all. The difference was sometimes in favour of one, and sometimes on the side of another; but the general result was that the largest quantity of gases was evolved upon the smallersurface of the wires.

715. Experiments of a similar kind were made with the single-plate, straight tubes (707.), and also with the curved tubes (708.), with similar consequences; and when these, with the former tubes, were arranged together in various ways, the result, as to the equality of action of large and small metallic surfaces when delivering and receiving the same current of electricity, was constantly the same. As an illustration, the following numbers are given. An instrument with two wires evolved 74·3 volumes of mixed gases; another with plates

73·25 volumes; whilst the sum of the oxygen and hydrogen in two separate tubes amounted to 73·65 volumes. In another experiment the volumes were 55·3, 55·3, and 54·4.

716. But it was observed in these experiments, that in single-plate tubes (707.) more hydrogen was evolved at the negative electrode than was proportionate to the oxygen at the positive electrode; and generally, also, more than was proportionate to the oxygen and hydrogen in a double-plate tube. Upon more minutely examining these effects, I was led to refer them, and also the differences between wires and plates (714.), to the solubility of the gases evolved, especially at the positive electrode.

717. When the positive and negative electrodes are equal in surface, the bubbles which rise from them in dilute sulphuric acid are always different in character. Those from the positive plate are exceedingly small, and separate instantly from every part of the surface of the metal, in consequence of its perfect cleanliness (633.); whilst in the liquid they give it a hazy appearance, from their number and minuteness; are easily carried down by currents; and therefore not only present far greater surface of contact with the liquid than larger bubbles would do, but are retained a much longer time in mixture with it. But the bubbles at the negative surface, though they constitute twice the volume of the gas at the positive electrode, are nevertheless very inferior in number. They do not rise so universally from every part of the surface, but seem to be evolved at different points; and though so much larger, they appear to cling to the metal, separating with difficulty from it, and when separated, instantly rising to the top of the liquid. If, therefore, oxygen and hydrogen had equal solubility in, or powers of combining with, water under similar circumstances, still under the present conditions the oxygen would be far the most liable to solution; but when to these is added its well-known power of forming a compound with water, it is no longer surprising that such a compound should be produced in small quantities at the positive electrode; and indeed the bleaching power which some philosophers have observed in a solution at this electrode, when chlorine and similar bodies have been carefully excluded, is probably due to the formation there, in this manner, of oxywater.

718. That more gas was collected from the wires than from the plates, I attribute to the circumstance, that as equal quantities were evolved in equal times, the bubbles at the wires having been more rapidly produced, in relation to any part of the surface, must have been much larger; have been there-

fore in contact with the fluid by a much smaller surface, and for a much shorter time than those at the plates; hence less solution and a greater collection.

719. There was also another effect produced, especially by the use of large electrodes, which was both a consequence and a proof of the solution of part of the gas evolved there. The collected gas, when examined, was found to contain small portions of nitrogen. This I attribute to the presence of air dissolved in the acid used for decomposition. It is a well-known fact, that when bubbles of a gas but slightly soluble in water or solutions pass through them, the portion of this gas which is dissolved displaces a portion of that previously in union with the liquid: and so, in the decompositions under consideration, as the oxygen dissolves, it displaces a part of the air, or at least of the nitrogen, previously united to the acid; and this proceeds *most extensively* with large plates, because the gas evolved at them is in the most favourable condition for solution.

720. With the intention of avoiding this solubility of the gases as much as possible, I arranged the decomposing plates in a vertical position (707. 708.), that the bubbles might quickly escape upwards, and that the downward currents in the fluid should not meet ascending currents of gas. This precaution I found to assist greatly in producing constant results, and especially in experiments to be hereafter referred to, in which other liquids than dilute sulphuric acid, as for instance solution of potash, were used.

721. The irregularities in the indications of the measurer proposed, arising from the solubility just referred to, are but small, and may be very nearly corrected by comparing the results of two or three experiments. They may also be almost entirely avoided by selecting that solution which is found to favour them in the least degree (728.); and still further by collecting the hydrogen only, and using that as the indicating gas; for being much less soluble than oxygen, being evolved with twice the rapidity and in larger bubbles (717.), it can be collected more perfectly and in greater purity.

722. From the foregoing and many other experiments, it results that *variation in the size of the electrodes causes no variation in the chemical action of a given quantity of electricity upon water.*

723. The next point in regard to which the principle of constant electro-chemical action was tested, was *variation of intensity.* In the first place, the preceding experiments were repeated, using batteries of an *equal* number of plates, *strongly* and *weakly* charged; but the results were alike. They were

then repeated, using batteries sometimes containing forty, and at other times only five pairs of plates; but the results were still the same. *Variations therefore in the intensity*, caused by difference in the strength of charge, or in the number of alternations used, *produced no difference as to the equal action of large and small electrodes.*

724. Still these results did not prove that variation in the intensity of the current was not accompanied by a corresponding variation in the electro-chemical effects, since the actions at *all* the surfaces might have increased or diminished together. The deficiency in the evidence is, however, completely supplied by the former experiments on different-sized electrodes; for with variation in the size of these, a variation in the intensity must have occurred. The intensity of an electric current traversing conductors alike in their nature, quality, and length, is probably as the quantity of electricity passing through a given sectional area perpendicular to the current, divided by the time (360. *note*); and therefore when large plates were contrasted with wires separated by an equal length of the same decomposing conductor (714.), whilst one current of electricity passed through both arrangements, that electricity must have been in a very different state, as to *tension*, between the plates and between the wires; yet the chemical results were the same.

725. The difference in intensity, under the circumstances described, may be easily shown practically, by arranging two decomposing apparatus as in fig. 12, where the same fluid is subjected to the decomposing power of the same current of electricity, passing in the vessel A between large platina plates, and in the vessel B between small wires. If a third decomposing apparatus, such as that delineated fig. 11. (711.), be connected with the wires at *a b*, fig. 12, it will serve sufficiently well, by the degree of decomposition occurring in it, to indicate the relative state of the two plates as to intensity; and if it then be applied in the same way, as a test of the state of the wires at *a′ b′*, it will, by the increase of decomposition within, show how much greater the intensity is there than at the former points. The connexions of P and N with the voltaic battery are of course to be continued during the whole time.

726. A third form of experiment in which difference of intensity was obtained, for the purpose of testing the principle of equal chemical action, was to arrange three volta-electrometers, so that after the electric current had passed through one, it should divide into two parts, which, after traversing each one of the remaining instruments, should reunite. The

sum of the decomposition in the two latter vessels was always equal to the decomposition in the former vessel. But the *intensity* of the divided current could not be the same as that it had in its original state; and therefore *variation of intensity has no influence on the results if the quantity of electricity remain the same.* The experiment, in fact, resolves itself simply into an increase in the size of the electrodes (725.).

727. The *third point*, in respect to which the principle of equal electro-chemical action on water was tested, was *variation of the strength of the solution used.* In order to render the water a conductor, sulphuric acid had been added to it (707.); and it did not seem unlikely that this substance, with many others, might render the water more subject to decomposition, the electricity remaining the same in quantity. But such did not prove to be the case. Diluted sulphuric acid, of different strengths, was introduced into different decomposing apparatus, and submitted simultaneously to the action of the same electric current (714.). Slight differences occurred, as before, sometimes in one direction, sometimes in another; but the final result was, that *exactly the same quantity of water was decomposed in all the solutions by the same quantity of electricity,* though the sulphuric acid in some was seventyfold what it was in others. The strengths used were of specific gravity 1·495, and downwards.

728. When an acid having a specific gravity of about 1·336 was employed, the results were most uniform, and the oxygen and hydrogen (716.) most constantly in the right proportion to each other. Such an acid gave more gas than one much weaker acted upon by the same current, apparently because it had less solvent power. If the acid were very strong, then a remarkable disappearance of oxygen took place; thus, one made by mixing two measures of strong oil of vitriol with one of water, gave forty-two volumes of hydrogen, but only twelve of oxygen. The hydrogen was very nearly the same with that evolved from acid of the specific gravity 1·232. I have not yet had time to examine minutely the circumstances attending the disappearance of the oxygen in this case, but imagine it is due to the formation of oxywater, which Thénard has shown is favoured by the presence of acid.

729. Although not necessary for the practical use of the instrument I am describing, yet as connected with the important point of constant electro-chemical action upon water, I now investigated the effects produced by an electric current passing through aqueous solutions of acids, salts, and compounds, exceedingly different from each other in their nature, and found them to yield astonishingly uniform results. But

many of them which are connected with a secondary action will be more usefully described hereafter (778.).

730. When solutions of caustic potassa or soda, or sulphate of magnesia, or sulphate of soda, were acted upon by the electric current, just as much oxygen and hydrogen was evolved from them as from the diluted sulphuric acid, with which they were compared. When a solution of ammonia, rendered a better conductor by sulphate of ammonia (554.), or a solution of subcarbonate of potassa was experimented with, the *hydrogen* evolved was in the same quantity as that set free from the diluted sulphuric acid with which they were compared. Hence *changes in the nature of the solution do not alter the constancy of electrolytic action upon water.*

731. I have already said, respecting large and small electrodes, that change of order caused no change in the general effect (715.). The same was the case with different solutions, or with different intensities; and however the circumstances of an experiment might be varied, the results came forth exceedingly consistent, and proved that the electro-chemical action was still the same.

732. I consider the foregoing investigation as sufficient to prove the very extraordinary and important principle with respect to WATER, *that when subjected to the influence of the electric current, a quantity of it is decomposed exactly proportionate to the quantity of electricity which has passed*, notwithstanding the thousand variations in the conditions and circumstances under which it may at the time be placed; and further, that when the interference of certain secondary effects (742. &c.), together with the solution or recombination of the gas and the evolution of air, are guarded against, *the products of the decomposition may be collected with such accuracy, as to afford a very excellent and valuable measurer of the electricity concerned in their evolution.*

733. The forms of instrument which I have given, figg. 9, 10, 11. (709. 710. 711.), are probably those which will be found most useful, as they indicate the quantity of electricity by the largest volume of gases, and cause the least obstruction to the passage of the current. The fluid which my present experience leads me to prefer, is a solution of sulphuric acid of specific gravity about 1·336, or from that to specific gravity 1·25; but it is very essential that there should be no organic substance, nor any vegetable acid, nor other body, which, by being liable to the action of the oxygen or hydrogen evolved at the electrodes (773. &c.), shall diminish their quantity, or add other gases to them.

734. In many cases when the instrument is used as a *comparative standard*, or even as *a measurer*, it may be desirable to collect the hydrogen only, as being less liable to absorption or disappearance in other ways than the oxygen; whilst at the same time its volume is so large, as to render it a good and sensible indicator. In such cases the first and second form of apparatus have been used, figg. 7, 8. (707. 708.). The indications obtained were very constant, the variations being much smaller than in those forms of apparatus collecting both gases; and they can also be procured when solutions are used in comparative experiments, which, yielding no oxygen or only secondary results of its action, can give no indications if the educts at both electrodes be collected. Such is the case when solutions of ammonia, muriatic acid, chlorides, iodides, acetates, or other vegetable salts, &c., are employed.

735. In a few cases, as where solutions of metallic salts liable to reduction at the negative electrode are acted upon, the oxygen may be advantageously used as the measuring substance. This is the case, for instance, with sulphate of copper.

736. There are therefore two general forms of the instrument which I submit as a measurer of electricity. One, in which both the gases of the water decomposed are collected (709. 710. 711.); and the other, in which a single gas, as the hydrogen only, is used (707. 708.). When referred to as a *comparative instrument*, (a use I shall now make of it very extensively,) it will not often require particular precaution in the observation; but when used as an *absolute measurer*, it will be needful that the barometric pressure and the temperature be taken into account, and that the graduation of the instruments should be to one scale; the hundredths and smaller divisions of a cubical inch are quite fit for this purpose, and the hundredth may be very conveniently taken as indicating a DEGREE of electricity.

737. It can scarcely be needful to point out further than has been done how this instrument is to be used. It is to be introduced into the course of the electric current, the action of which is to be exerted anywhere else, and if 60° or 70° of electricity are to be measured out, either in one or several portions, the current, whether strong or weak, is to be continued until the gas in the tube occupies that number of divisions or hundredths of a cubical inch. Or if a quantity competent to produce a certain effect is to be measured, the effect is to be obtained, and then the indication read off. In exact experiments it is necessary to correct the volume of gas for

changes in temperature and pressure, and especially for moisture*. For the latter object the volta-electrometer (fig. 11.) is most accurate, as its gas can be measured over water, whilst the others retain it over acid or saline solutions.

738. I have not hesitated to apply the term *degree*, in analogy with the use made of it with respect to another most important imponderable agent, namely, heat; and as the definite expansion of air, water, mercury, &c., is there made use of to measure heat, so the equally definite evolution of gases is here turned to a similar use for electricity.

739. The instrument offers the only *actual measurer* of voltaic electricity which we at present possess. For without being at all affected by variations in time or intensity, or alterations in the current itself, of any kind, or from any cause, or even of intermissions of action, it takes note with accuracy of the quantity of electricity which has passed through it, and reveals that quantity by inspection; I have therefore named it a Volta-electrometer.

740. Another mode of measuring volta-electricity may be adopted with advantage in many cases, dependent on the quantities of metals or other substances evolved either as primary or as secondary results; but I refrain from enlarging on this use of the products, until the principles on which their constancy depends have been fully established (791. 843.).

741. By the aid of this instrument I have been able to establish the definite character of electro-chemical action in its most general sense; and I am persuaded it will become of the utmost use in the extensions of the science which these views afford. I do not pretend to have made its detail perfect, but to have demonstrated the truth of the principle, and the utility of the application.

[To be continued.]

XXVI. *On the Internal Structure of Plants. By the Rev.* Patrick Keith, *F.L.S.*

[Continued from p. 121.]

Composite Organs.

The Epidermis.—THE epidermis of the vegetable, a term borrowed from the anatomy of animals, is the external envelope or integument of the plant, extending over its whole surface, and covering the root, stem, branches, leaves, flower, and fruit, with their appendages, the summit

* For a simple table of correction for moisture, I may take the liberty of referring to my Chemical Manipulation, edition of 1830, p. 376.

of the pistil only excepted. But although it is extended over the whole of the plant's surface, it is not of the same tenuity throughout. In the root and trunk it is in many plants a tough and leathery membrane, or it is a crust of considerable thickness; while in the leaves, flowers, and tender shoots, it is a fine colourless and transparent film not thicker than a cobweb. It is colourless, however, only when detached; for when adherent, it assumes the colour of the parts immediately beneath it. Hence the green colour so prevalent in the leaf and tender shoot, and the beautiful variety of hues displayed in flowers and fruits.

Du Hamel, who seems to have been the first to study its structure minutely, describes it as being formed of a multiplicity of fine and delicate fibres, placed in a parallel direction, but inosculating at regular intervals, so as to constitute a network, the meshes of which are filled up with a thin and transparent pellicle—single, as in the epidermis of the leaf; or divisible into several layers, as in the stem of the Paper Birch *Betula papyracea*, in which you may count six or more*.

Saussure the elder inspected it as it occurs in the leaves and petals of Jessamine and Foxglove, and describes it as constituting a bark composed of two layers, the interior layer being net-like, and interspersed with a multiplicity of what he calls cortical glands, and the exterior layer being totally destitute of organization†.

Hedwig describes it as forming a network of fibres that consists of two distinct but adherent *laminæ*; but he regards the cortical glands of Saussure as being merely pores or apertures perforating the pellicle that fills up the mesh‡.

Comparetti describes it as consisting of a network of fibres ascending in an oblique direction, and forming hexagonal meshes of various sizes and positions, the area of the meshes being occupied by opake or transparent points, of an oval or roundish figure, that seem to be somewhat inflated, as if filled with air or water. He studied it chiefly as it occurs in the leaves of succulent plants §.

The above descriptions do not, indeed, tally quite so completely as could be wished; but an exact coincidence was not to be expected in the description of an organ that differs so much in different species of plants, and even in different parts of the same plant. They agree in all that is essential. Whoever will be at the trouble to repeat the observations will find that the foregoing descriptions exhibit a sufficiently cor-

* *Phys. des Arb.*, liv. i. chap. ii. † *Observations sur l'Ecorce de Feuilles.*
‡ Tracts relative to Botany, 1805. § Senebier, *Phys. Veg.*

rect view of the general aspect of the epidermis; and if he extends his researches further, he will very probably meet with varieties of structure different from any that have been yet specified. Nature loves to luxuriate in varieties, and further varieties have been accordingly met with.

Mr. F. Bauer, of Kew, describes the cuticle of *Doryanthes hastata Corrêa* as consisting of two or three stories of cells laid one above another, and exhibiting in their aggregate aspect a resemblance to that of a honeycomb. The epidermis of the inner surface of the petals of *Crocus vernus* presents the similitude, neither of a network of fibres, nor of stories of minute cells, but of a thin and individual layer of parallel and tangent reeds of unequal lengths, interspersed with multitudes of minute and shining points, or molecules, and resembling a front view of the false pipes of an organ. Finally, in the stem and branch of woody plants, the epidermis often exfoliates, and is again regenerated even if destroyed by accident; but in herbaceous plants, and in the leaf, flower, and fruit of other plants, the epidermis never exfoliates, and is never again regenerated if once destroyed.

The Pulp.—The pulp, or cellular tissue, is a soft and succulent substance, constituting the principal mass of herbaceous plants, and a notable proportion of many parts even of woody plants. It abounds in the seed-lobes and in succulent fruits, of which any one may easily satisfy himself by cutting up, whether in a longitudinal or transverse direction, a bean or an apple recently gathered from the stalk or tree. It is also particularly conspicuous in the leaf and flower, with their footstalk, when stript of the epidermis. Nor is it wanting even in the stem of woody plants, though it is cognizable, at least as a separate organ, only in the pith or in the bark of the young and tender shoots, where it constitutes a thin layer immediately under the epidermis, and forms a sort of secondary integument to the plant, known among botanists by the name of the cellular integument. In the leaves its colour is generally green, and in the seed-lobes white; while in flowers and fruits it assumes almost all varieties of shade, according to the species of plant, or according to the circumstances in which it is placed. When viewed without the microscope its appearance is that of an assemblage of small and minute granules, imbedded in a soft and glutinous substance, as in the greater part of leaves and succulent fruits. But it is only when viewed minutely with a good glass that its true structure is to be detected. Malpighi describes it with his usual accuracy, and compares it to an assemblage of inflated threads or bladders, containing a juice. Grew describes it under the appellation

of the parenchyma, and compares it to the bubbles formed upon the surface of liquor in a state of fermentation. Du Hamel represents it as consisting of a sort of network of fibres, crossing in all directions, and interspersed with small and granular, or bladder-like substances occupying the interstices.

Such are the descriptions of the earlier vegetable anatomists, in one or other of which the general appearance of the pulp will be found to be pretty fairly represented under whatever aspect the botanical student may happen to meet with it. But later anatomists have been more minute. Mirbel describes it as being composed of clusters of small and hexagonal cells, containing a juice. This was an important step in advance, yet he obscures his description in the introduction of a useless distinction, by which he divides his cellular tissue into a parenchyma and a herbaceous tissue, the former containing a coloured, the latter a colourless, juice. But an apparatus of united cells, containing a fluid whether colourless or coloured, is all that is necessary to form a true pulp. The cells he does not regard as being distinct and individual organs, such as might be insulated and shown separately, but merely as being formed of a fine and delicate membrane so folded or doubled up as to leave the partitions single and common to two cells. This doctrine Dutrochet has shown to be groundless, and if we repeat his experiment we shall come to the same conclusion. Take a portion of pulp and put it into a phial filled with nitric acid. Plunge the phial thus filled into boiling water, and the cells will soon become easily detachable, or will soon begin to separate and to present themselves entire in their hexagonal form. Hence, where the walls touch, the membranes must be double *.

Further, Mirbel regards the partitions of the cells as being perforated by minute holes, or pores, for the transmission of sap or other juices of the plant. Yet his doctrine of internal and visible pores has not been generally adopted by other vegetable anatomists. It was attacked and denied, rather rudely, by the German doctors Sprengel and Treviranus, and again affirmed and reasserted by Mirbel in his *Défense de ma Théorie*, as well as by Link, Hedwig, and Rudolphi. But as the pores in question have been still more recently and more laboriously searched for by M. Dutrochet without success, I suppose we must be content to regard the doctrine as unfounded. Dutrochet says the pores are merely small globules imbedded in the walls of the cells†. It should

* *Recherches Anatomiques*, tom. xi. † *Ibid.* tom. xiii. p. 40.

be added, however, that Mirbel could not account for the fact of the lateral transmission of sap, except through the channel of visible pores; while Dutrochet, by means of the agency of molecular infiltration, can account for it very well without them *.

The Pith.—The pith, as has been already shown, is a soft and spongy, but often succulent substance, occupying the centre of the root, stem, and branches, and extending in the longitudinal axis of the plant, in which it is inclosed as in a tube. Its structure is similar to that of the pulp, being composed of an assemblage of hexagonal cells, containing for the most part a watery and colourless juice. Mirbel regards it as being furnished with pores like those already described; but Dutrochet denies their existence, and contends that the fancied pores are merely minute molecules imbedded in the walls of the cells like those of the pulp. He designates them by the name of nervous corpuscles †; and affirms, besides, that there is no pith in the root ‡. In many roots, we admit that there is no visible pith; but let any one examine the root of *Acorus Calamus* or *Berberis communis*, and then let him say what he thinks of the affirmation.

Yet, if its structure is so very similar to that of the pulp, why, it may be said, is it to be designated by another name? The central situation of the pith is, perhaps, of itself a sufficient reason. But there seems to be besides a difference of texture in the membranes composing them. Let the juiceless pith of the Elder or of the Bulrush be compared with the withered pulp or cellular integument of the Lime-tree, and the difference will be obvious.

The Cortical Layers.—The cortical layers, or interior and concentric layers constituting the mass of the bark, are situated immediately under the cellular integument, where such integument exists, and where not, immediately under the epidermis, or they are themselves external. They are distinguishable chiefly in the bark of woody plants, but particularly in that of the Lime-tree, in which they are easily separated by maceration or exposure to the weather, and in which you may readily count a dozen or more in a branch or trunk of any considerable size.

In aged trunks the outer layers are coarse and loose in their texture, exhibiting individually a conspicuous and considerably indurated, but very irregular network, composed of bundles of longitudinal or cortical fibres, not ascending the stem directly, but winding more or less around the axis of

* *Recherches Anatomiques*, p. 48. † *Ibid.* p. 13. ‡ *Ibid.* p. 46.

the plant. As the layers recede from the circumference, the network which they form is finer, though still very irregular, and their texture more compact. Yet the meshes of the different layers often correspond, and form, at least in aged trunks, pyramidal apertures, or widen into large gaps or chinks, as in the trunk of the Oak or Elm, exhibiting still the rough traces of the original network. In young trees or shoots the apertures formed by the coincidence of the meshes are not yet left empty, but are occupied by a pulp, somewhat compressed, which traverses the longitudinal fibres, and binds and cements them together.

In all trunks the inner layers are soft, smooth, and flexible, and capable of subdivision till reduced to an absolute film, but not always exhibiting a conspicuous network. The innermost layer of all is denominated the *liber*, the Latin term for a book, from its having been used by the ancients to write on before the invention of paper*. It is the finest and most delicate of the layers, and is often most beautifully reticulated, as in the liber of *Daphne Lagetto*, remarkable beyond that of all other plants for the beauty and delicacy of its network, and soft and flexible as the finest lace. If the cortical layers while yet young are accidentally injured, the part destroyed is again regenerated, and the wound healed up without a scar; but if the wound extends beyond the liber, the part destroyed is no longer regenerated.

The Ligneous Layers.—The ligneous layers, or layers constituting the wood, occupy the intermediate portion of the stem between the bark and pith, and are distinguishable into two different sets, concentric layers and divergent layers.

The concentric layers, which constitute by far the greater part of the mass of the wood, may be seen on the surface of a horizontal section of almost any trunk or branch, as on that of the Oak or Elm, particularly after being for some time exposed to the weather. They have been believed to be equal in number to the years of the plant's growth; but they are not literally or strictly so. Neither are they literally or strictly concentric. On the one side or on the other there is generally an excess of width as well as of number, not according as it is exposed to, or sheltered from, the light and heat of the sun, as some writers have affirmed, but according to the accidental situation of the great roots and branches. The inner layers are the hardest and the outer layers the softest, and the outermost layer, which is the softest of all, is

* Is not the converse of this the fact—Was not *a book* called *liber* because it consisted of the vegetable substance to which that appellation was originally confined?—Edit.

denominated the *alburnum*, till in the process of vegetation it becomes an inner layer, in its turn, more solid and more condensed, and is ultimately converted into perfect wood.

The divergent layers, which intersect the concentric layers in a transverse direction, constitute also a considerable proportion of the wood, as may be seen on a horizontal section of the stem of the Fir or Birch-tree, on which they present an appearance like that of the radii of a circle. But if the wood is split longitudinally, fragments of the divergent layers will be seen adhering to the surface of the fracture, in the form of large and smooth plates, which interweaving themselves among the concentric layers, in the manner of an irregular wickerwork, form a sort of tense binding, that cements and unites the whole. This appearance is peculiarly conspicuous on the riven surface of the Elm-tree or of the Oak.

In following up the analysis of the ligneous layers you cannot separate the two sets, so as to exhibit each of them entire; but as the divergent layers are soluble in certain fluids in which the concentric layers are not soluble, the latter may be exhibited pretty entire by means of the destruction of the former. That which seems at first sight to be merely an individual layer, proves upon further inspection to be a group consisting of component layers, finer and smaller still, till at last you can follow the division no further. Du Hamel macerated a piece of the trunk of an Oak-tree in water, with a view to dissolve the divergent layers, and found, after a long time, that the minuter divisions of the concentric layers consisted ultimately of an assemblage of longitudinal or woody fibres, so as to form a network similar to that of the *liber*. The root of the Artichoke, after being pulled up out of the soil, and exposed for a considerable time to the action of the atmosphere, affords, perhaps, the most beautiful of all examples of this sort. It separates, thus, into thousands of layers of network, each as fine and as delicate as a piece of Brussels lace.

Yet this mode of analysis gives us no knowledge of the structure of the divergent layers. We must consequently have recourse to the aid of the microscope. Take a minute slice of a divergent layer from the riven surface of an Oak or Elm, and put it under a good glass, and you will find that it has the appearance of being composed of an assemblage of parallel fibres, or threads of contiguous vesicles not forming a network, but closely crowded together, and compressed into a thin plate. It is apparently nothing more than the vesicles or cellular tissue of the pulp that existed originally in the *alburnum*, now deprived of its parenchyma, or contained fluid,

but still filling up the interstices of the concentric layers, and binding them together, according to the similitude of Grew and Malpighi, as the woof of a web binds together the longitudinal threads of the warp.

[To be continued.]

XXVII. *Considerations relative to an interesting Case in Equations.* *By* W. G. Horner, *Esq.**

PERCEIVING that Professor Moseley has resumed the development of the principle of least pressure, I presume that the discussion, which was introduced by Mr. Earnshaw, respecting the validity of the principle, has terminated. Without entering, therefore, upon the general question, in the fate of which I have no other interest than every lover of science must be supposed to have, I may be allowed to express my disappointment at the unsatisfactory result of that portion of the argument which, if conclusively handled, was likely to have proved the most impressive. I allude to that passage in which the Rev. Professor's reasoning assumed the tangible form of an equation. That this was regarded by both the disputants as a critical point, is abundantly apparent; and, in fact, if the general theory is "such, in its nature, as cannot be submitted to the test of experiment†", it is doubly requisite that the testimony of calculation should be clear. For these reasons, the liberty which I use in recalling attention to that particular point will be the more readily excused by the gentlemen who have already agitated the question.

In page 200 of the Number of this Magazine already referred to, Mr. Moseley, in considering the case of "a pressure equally divided between *three* points of support in the same right line," gives the following as "the two equations of equilibrium:"

$$\text{“}\frac{\alpha}{x} + \frac{\alpha}{x-b} + \frac{\alpha}{x-c} = M$$

$$\frac{\alpha b}{x-b} + \frac{\alpha c}{x-c} = M a.\text{”}$$

The resultant of this pair of equations he finds, with the tacit assent of Mr. Earnshaw, to be a mere *quadratic*, which, however, seems to have been regarded by each party as having but one root; and in consequence of this complicated oversight, the dispute settled down into a discussion of the point of legitimacy

* Communicated by the Author.

† See Phil. Mag. and Annals, March 1834, p. 194.

between two roots, neither of which has been proved to be relevant to the argument. What is still more strange, that root which has been most strenuously claimed as auxiliary to one side of the cause, proves to be quite adverse to it. Under all the circumstances, it is not without a sentiment approaching to diffidence that I venture to make these assertions; and especially because the objections I have to offer to the method employed in reducing the proposed equations are so palpable, that it is difficult to dismiss the notion that they have been considered and overruled; but on what sound principle of reasoning I cannot conjecture.

1. If the equations contain but one unknown quantity, one of them suffices for the solution, and the other is either superfluous or contradictory; and so, *à fortiori*, is the third equation, which has been derived from them.

2. If two unknowns are involved in each equation, either two new equations must be formed by elimination, or if only one subsidiary equation is employed, the result must at all events be introduced into the original statements.

3. A third exception applies to the mode of obtaining the subsidiary equation, namely, by taking the quotients of the separate scales. If it were stated that $x^3 = a$, and $x = b$, would it follow that $\therefore x^3 = \frac{a}{b}$, and $x = \pm\sqrt{\frac{a}{b}}$? Assuredly not.

4. This objection is still more valid when the quantities exterminated by division are zero or infinite. That this impediment exists in the present instance will appear on adopting a mode of reduction exempt from the faults above recited; *e. g.*

Dividing by α, the equations become

$$\frac{1}{x} + \frac{1}{x-b} + \frac{1}{x-c} = \frac{M}{\alpha} \quad \cdots \quad \cdots \quad (1.)$$

$$\frac{b}{x-b} + \frac{c}{x-c} = \frac{Ma}{\alpha} \quad \cdots \quad \cdots \quad (2.)$$

Deducting (2.) from a times (1.),

$$\frac{a}{x} + \frac{a-b}{x-b} + \frac{a-c}{x-c} = 0 \quad \cdots \quad \cdots \quad (3.)$$

To prove this to be a complete *cubic* equation, it would be abundantly sufficient to make $x = \frac{1}{z-\gamma}$, and reduce. The

result would be a cubic, complete in all its terms. In fact, merely say $x = \frac{1}{z}$, and we have

$$\left(a + \frac{a-b}{1-bz} + \frac{a-c}{1-cz}\right) z = 0 \quad \ldots \quad (4.)$$

which, when cleared of fractions, will be an equation of *three* dimensions.

If the parenthetic portion of (4.) is made $= 0$, and reduced, the result will be a quadratic agreeing with Mr. Moseley's. But here is, besides, a third root, $z = 0$, which gives $x = \frac{1}{0}$ and $\frac{M}{\alpha} = 0$ in each equation (1.), (2.). Q. E. D.

Again, this infinite x not only solves both the original equations, it does so to the exclusion of any other infinite x presumed to be deducible from any relation incident to a, b, c. For it reduces (1.) (2.) (3.) to *simple* and *dependent* equations, viz. $\frac{3}{x} = 0$, $\frac{b+c}{x} = 0$, $\frac{3a-b-c}{x} = 0$, which all merge in $\frac{1}{x} = 0$; the condition $3a-b-c = 0$ being quite superfluous and nugatory.

In some views of the equation $\frac{M}{\alpha} = 0$, upon which the infinity of x depends, physical and analytical considerations are inseparable, and results are obtained which confirm what has just been alleged; *e.g.* it distinctly announces either that the *mass* M has *absolutely no* weight, or else that the standard moment of *pressure* α is *infinite*. The latter is of course the alternative to be preferred, as it cannot be Mr. Moseley's design to discuss the relations of weight and pressure in masses absolutely destitute of weight. But if α is infinite, what becomes of its constancy? "Let α represent a constant quantity." Or, granting that, what becomes of the quantities $A = \frac{\alpha}{x}$, $B = \frac{\alpha}{x-b}$, $C = \frac{\alpha}{x-c}$? Can they be severally affirmative and infinite, although their sum is = the finite quantity M? If not, x must have been infinite from the first, and irrespectively of a, b, c.

After all, the only way of arriving at conclusions perfectly satisfactory is by regular elimination; the labour of which, even by the easiest methods, is in this instance considerable. I have, however, undertaken it, not only for the sake of epi-

tomizing the present argument, but in the hope of supplying students with a pretty addition to their collections of examples. Reducing (1.), (2.), and making $y = \frac{a}{M}$, $s = b + c$, $p = b\,c$, the equations to be solved are

$$x^3 - (s + 3y)x^2 + (p + 2sy)x - py = 0 \ldots \quad \ldots \quad (A.)$$

$$x^2 - \left(s - \frac{sy}{a}\right)x + \left(p + \frac{2py}{a}\right) = 0 \ldots \quad \ldots \quad (B.)$$

Eliminating y by the method of the common measure, we find

$$(3a - s)x^2 - 2(as - p)x + ap = 0 \quad \ldots \quad \ldots \quad (C.)$$

as determined by Mr. Moseley. But if we also eliminate x by the method of combinations, we obtain a formula in y which, when arranged, resolves itself into the factors

$$\frac{(4p - s^2)py^2}{a^3} = 0 \quad \ldots \quad \ldots \quad \ldots \quad (D.)$$

and

$$3(3a - s)y^2 - 2(3a^2 - 2as + p)y + (a^2 - as + p)a = 0 \quad (E.)$$

Of these, equation (D.) shows that $y = 0$ yields two solutions, which are readily found to be $x = b$, $x = c$. But when these values are placed in (1.), (2.), the conditions appear to be rather eluded than satisfied, the equations being reduced to a balance of infinity.

Equations (C.) (E.) being compared, in the event of $s = 3a$, which leads to infinite values of x and y, which answer to each other, and finite values which likewise correspond, give in the former case $x = 3y$. The same result is obtained directly from either (A.) or (B.), their mutual independence and their dependence upon a, b, c, being simultaneously destroyed by the same hypothesis, viz. that of the infinity of x, necessarily involving that of y.

The corresponding finite values are $y = \frac{p - 2a^2}{3a^2 - p} \cdot \frac{a}{2}$, and $x = \frac{ap}{2(3a^2 - p)}$, which appear, therefore, to furnish the only legitimate solution of the pair of equations in the event which has been insisted upon.

It is for Mr. Moseley to determine how far his principle is affected by these results. My concern has been to solve a curious difficulty connected with equations in a case where the application of some of the ordinary rules has proved illusive. The general equation, of which (3.) is a partial case, deserves attention.

XXVIII. *Observations on the Spectra of the Eye and the Seat of Vision. By Mrs.* GRIFFITHS.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE main object which induced me to communicate the curious discovery of the vision of the retina, was to elicit the attention of the English philosophers to that part of it which related to the *seat of vision*. This question has been lost sight of entirely; and although I have been gratified with the zeal with which Sir David Brewster has pursued the experiment, yet it has taken a different course from the one I so much desired.

In the remarks on my communication, the Editor observes that "some of the conclusions in the paper, especially those about the seat of vision, are not correct." It is not specified in what the error consists. I assert on good grounds that *the office of the retina is to contract and dilate the pupil.* When the pupil contracts, the intersections or meshes of the retina are elongated, and of course they are *thinner*, and the interstices or squares between are larger. When the pupil dilates, the lines, or meshes, or intersections, whatever they may be called, are *thicker*, and the spaces between are smaller. Surely this proves that the retina is of an elastic nature, and its office is sufficiently well defined.

Is it philosophical to suppose that a part of the eye so singularly constructed can transmit the impressions of external objects to the sensory? It is composed of squares, with regular intervals of interstices, and these squares are for ever varying with every change of light; in which way, therefore, could this separate apparatus transmit all the various forms which are presented by external objects? Perhaps there is no portion of the eye so little adapted to the purpose of transmitting objects or impressions as the retina, and I would enlarge on this point did I not think that some abler pen would soon appear to discuss the matter in a fuller form. I wish the fact to be established, that the seat of vision does not belong to any one part of the eye in particular, but that the whole apparatus of the eye is necessary to the conveyance of external impressions to the sensory.

I consider the eye, as it respects mental vision, as an external object, and that under different modifications of light every portion of it can be seen. For instance, Mr. W. G. Horner can see the "blood-vessels of the retina." I can see the retina, the opening of the pupil, the crystalline lens, the air-bubbles

* See Lond. and Edinb. Phil. Mag. vol. iv. pp. 43, 115, 241, 262, and 354.—EDIT.

in the aqueous chamber; the aqueous humour itself flowing in and circulating incessantly; the fixed spots on the outer surface of the crystalline lens; and the connected floating links of air-bubbles, which, when numerous, cause the disease in the eye called amaurosis *.

The whole apparatus of the eye is therefore, to the *mind*, of no more importance than the internal machinery of a watch is to the hour-hand of the dial-plate. The machinery of the watch and of the eye is in constant motion, but no one part of either is the direct cause of the movement of the hour-hand, or of the perception of impressions.

The mind sees the time as specified by the motion of the hands, and the mind has the perception of impressions as specified by the simultaneous operations of the different portions of the eyeball. By *inspection*, all the machinery of the watch is visible to the mind, and by *inspection*, all the machinery of the eye is visible to the mind likewise.

The impression of external objects is instantaneous, and is not transmitted from the object to any one particular part, and thence to the sensory. Vision is impaired when any one part of the eye is diseased, although the retina, so universally admitted to be the seat of vision, is in a healthy state.

The physiological or mechanical seat of touch lies at the end of the fingers, at the ends of the toes and tongue, and in the lips, that is, the qualities of bodies are rendered perceptible to the sensory through the agency of these parts of the body. No one supposes that the seat of touch resides in any particular part of the skin itself, whether finger, tongue, or lip. The sensation of touch is produced by direct pressure, a pressure which is instantaneous, which commences at the point of contact, and ends in the brain, whence the mind takes immediate cognizance of it.

So it is with all the senses; there is an instantaneous communication between the outward points which come into contact with tangible bodies and their qualities, and the central point in which consciousness dwells.

Seeing, being the most complicated of all the senses, requires a more complicated apparatus to convey impressions to the mind. The different parts are for the admission, the modification, and the absorption of light, whilst amongst this curious machinery the adjustment and continued action of these several parts are maintained by certain muscular and valvulous

* We apprehend that this is not the case. Although it is certainly stated by some medical writers that the appearance of these *muscæ volitantes*, as they are termed by pathologists, indicates in certain cases the commencement of *amaurosis*, yet they do not, we believe, actually constitute any form of that disease.—Edit.

movements, all necessary to the due transmission of impressions from without.

It is not, therefore, that part of the machinery of the eye called the retina which first receives the impressions from without, although it is an expansion of what is called the optic nerve. There is still an agent unknown to us, which, in all cases of the different senses, conveys the impression of external bodies and their qualities to the sensory or consciousness. The retina acts no more in the transmission than what is effected by the pressure of the finger when we wish to inform the mind of the quality of any body. For if the contractile power which conveys the sensation of qualities be not likewise an expansion of the same nerve, originating in the same central point whence the optic nerve emanates, yet it most assuredly operates by virtue of the same principle or power which transmits the different sensations that the qualities and appearances of bodies produce.

Of one thing we are now certain, that the seat of vision lies beyond the machinery of the eyeball; for by repeated observation and experiment I can see almost every portion of it. This does not arise from any imperfection or obliquity of vision; for my eyes are, and always have been, in a healthy sound state. I am only sorry that I did not commence the investigation at an earlier period. It remains still a matter of doubt with me, whether an eye younger than forty can be sensible to the appearances which those beyond that age are capable of comprehending. I do not say that the eye must be injured by disease, for old age is not a disease, but I suspect that an eye in full vigour has all its parts so nicely adjusted, and the elasticity is so on the *qui vive*, that there is an intimate adhesion and blending of the whole machinery, no one portion of which is more relaxed than the other.

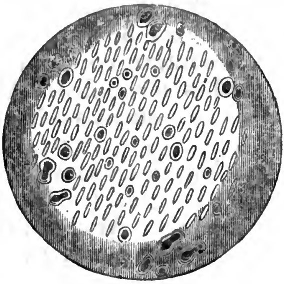
Nine or ten years ago I discovered that, by placing the light of a candle at a certain distance from the eye, and allowing it to fall obliquely—as I stated in p. 306. of Our Neighbourhood*—on the cornea, I could see part of the interior of my own eye. I recollect drawing a map of the spectrum as it invariably appeared, and several years after I showed it to Dr. Hosack. Since that period I can bring the spectrum to my mind either with the eye half closed or nearly open, either by means of a candle, or from a ray of sun-light. I subjoin a map of the spectrum as it appears in both eyes, for, as happens to Mr. Horner, each eye presents a somewhat different appearance.

The spots in the margin are dark in the centre, with a halo of paler light around them. The golden appearance of the

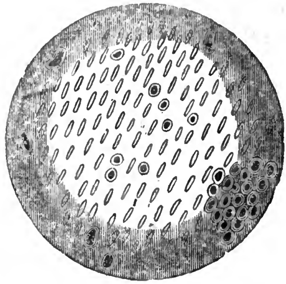
* The title of a Novel by Mrs. Griffiths.

oblong drops in the centre answers to what Mr. Horner calls "golden rain"; but the brilliancy and golden hue originate in the colour of the light, for a ray of the sun gives it a silver colour. I have described the whole in p. 306. of Our Neighbourhood. At first, before I could keep my eye steady, I only saw the quarter, and oftentimes a very small sector, of the spectrum; now I see the whole at once.

Right Eye.

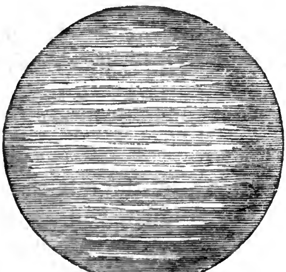


Left Eye.

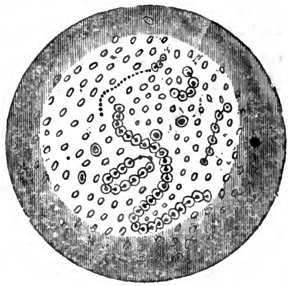


In front of this spectrum are to be seen, as below in No. 1. and No. 2., the loose air-bubbles and connected links, all moving various ways and changing their position and connexion with one another every instant.

No. 1.



No. 2.



In No. 1. I have merely represented the water which flows in the aqueous chamber; in No. 2. I have represented the transparent air-bubbles, and the links of these same bubbles. They are sometimes so fine as to have the appearance of a transparent cambric thread. All these are perpetually floating about in the fluid of No. 1., which is in constant motion, rising and falling like waves.

In a future communication I shall give you these images very highly magnified, but at present I thought it better to show them as they appear by the aid merely of the magnifying

power of the eye itself. As soon as sufficient attention has been excited towards this curious phænomenon, I shall give the world a new discovery in this branch of optics, one so very curious that it deserves to be ushered into notice when the mind has contemplated all the preparatory steps which your knowledge shall make familiar.

It is not mere speculation when I assert that the office of the retina is to regulate the admission of light. I go on safe grounds: experiment has convinced me that the lines which compose the meshes of the retina are thicker or thinner as the opening of the pupil varies. In some future communication I will show you one of these meshes magnified. Mr. Horner is correct in saying that the eye sustains no injury in making these experiments.

The brilliancy of colouring and the kaleidoscope patterns of which Sir David Brewster speaks, belong exclusively to the peculiar shape and structure of the retina. With all the multiplied and various experiments that I have made, I have observed that all the brilliant as well as opake figures are square or angular. I have never seen anything approaching to circularity but the stars which I have described in my first paper. That star, or those stars, for there is one in the centre of each square or mesh, may be in reality composed of points or angles, but they are too minute to ascertain this fact. They scintillate.

XXIX. *Addendum to a Descriptive Catalogue of the Minerals of the North of Ireland.* *By* JAMES BRYCE, *Jun., M.A., F.G.S., &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN a note appended to a paper of mine on the minerals of the North of Ireland, published last August, you inquire, in relation to a new mineral, which Dr. Thomson has named hydrocarbonate of lime and magnesia, "Whether this mineral is allied to that which the late Mr. W. Phillips, at the suggestion of Mr. Brayley, described under the appellation of Hydrocarbonate of Lime, in the last edition (1823) of his Elementary Introduction to Mineralogy, p. 161." It is then added, from a communication by Mr. Brayley, that the latter mineral "is the result of the action of the trap dykes of the Giants' Causeway upon the chalk which they have intersected, and, according to the analysis of the late Dr. Da Costa, would appear to be composed of four atoms carbonate of lime and there atoms water," &c. In reply I may observe, that I believe the minerals to be distinct, though obviously closely al-

lied. That analysed by Da Costa occurs wherever the trap dykes cut the chalk*. Dr. M'Donnell, who first noticed this mineral, informs me, that it was pronounced to be dolomite by several skilful mineralogists and chemists to whom he showed it at various times. The analysis of Da Costa, however, gave no magnesia. The other mineral yielded to Dr. Thomson some portion of that earth. It has been found only at Downhill in Derry, in veins and irregular masses in an amygdaloid of a loose texture, accompanied by zeolites and the common carbonate of lime. On these grounds I think the two minerals may be regarded as distinct; an analysis of the magnesian one will most probably be given in full in the work on mineralogy for some time expected from the pen of the distinguished chemist just named. In the System of Professor Mohs, and the excellent "Manual," by Robert Allan, Esq., just published, this and several others, usually described as distinct, are classed under calcareous spar†.

It was stated in the "Catalogue," that the large crystals of quartz so frequent at Dungiven were found in a trap rock. I believe this is inaccurate. Some of the crystals are found in the bed of the river Roe, which is partly primitive; others are found in the debris of mountains of the basaltic range, but have most probably been transported thither by currents from the primitive country to the west. Small crystals of quartz, however, are frequent in our trap rocks. Hydrolite and levyne have been recently found in other places within the basaltic district besides Island Magee, and anhydrous disilicate of iron in dykes and loose boulders of trap near Larne. Mr. M'Adam suggests that it ought to have been mentioned in the "Catalogue," as a remarkable circumstance, that the magnetic or octahedral iron ore found by him in the Isle of Muck, occurs only on the external surface of the rock.

The researches of Lieut. James, R.E., in the county of Down, and the zealous labours of Mr. Patrick Doran in all

* It perhaps ought to be observed, that this mineral may have been found in the vicinity of the Causeway, but certainly not immediately there; because there is no chalk at the Causeway, nor, so far as I am aware, at any place in the trap district where columnar basalt exists. In such a case that rock rests on lias or sandstone.

† [We are obliged to Mr. Bryce for his reply to our inquiry. We do not know what edition of Prof. Mohs's System he may refer to, but in Haidinger's Translation neither of the minerals in question is adverted to, either by name or by implication; nor would it be in accordance, we think, with Prof. Mohs's principles of classification, to include them "under calcareous spar": the other minerals "usually described as distinct" but which are also regarded by that mineralogist as varieties of calcareous spar, consist, essentially, like that mineral, of carbonate of lime alone, and are also connected with it into one species by gradual transitions of external character, or by what in Zoology and Botany is termed *affinity*.—E. W. B.]

parts of the country, have added some new and very interesting minerals to our former catalogue. The following is a list of these:

Variolite.—This variety of compact felspar has been lately met with in the hornblende rock of Morne.

Anthracite.—A compact variety of this mineral, with a highly metallic lustre, occurs frequently in the grauwacké of Down.

White Carbonate of Lead—accompanies galena and the green phosphate of lead in the Newtonard's lead-mine, Down.

Colophonite.—This mineral occurs in quartz veins traversing siliceous slate near Glassdrummond, Morne. It is of a brownish yellow colour, and is crystallized in rhombic dodecahedrons, with very unequal angles, and having striæ parallel to the lesser axis of the rhomboid. The mineral has very much the appearance of cinnamon stone.

Sulphuret of Molybdena.—This mineral has been lately discovered in a siliceous slate, or, I believe, rather a chlorite slate, on the shore near the mountains of Morne. The crystalline form is a six-sided table, terminated by a low six-sided pyramid, of which the base angles are truncated. Neither this mineral nor the colophonite have, so far as I know, been before noticed in Ireland. Yours, &c.

Belfast, Aug. 11, 1834. JAMES BRYCE, Jun.

XXX. *On the Development of certain Trigonometrical Functions.* By J. R. YOUNG, *Professor of Mathematics in the Royal College, Belfast.**

THE series given by analytical writers for the development of a circular arc in terms of its sine, cosine, and tangent are as follows:

$$\sin^{-1} x = x + \frac{x^3}{1.2.3} + \frac{3^2 x^5}{1.2.3.4.5} +, \text{\&c.}$$

$$\cos^{-1} x = \frac{\pi}{2} - x + \frac{x^3}{1.2.3} - \frac{3 x^5}{1.2.3.4.5} +, \text{\&c.}$$

$$\tan^{-1} x = x - \frac{x^3}{3} + \frac{x^5}{5} - \frac{x}{7} +, \text{\&c.}$$

which series, as well as those for the development of the arc in terms of the other trigonometrical lines, are true only for the least of the arcs to which these trigonometrical lines belong. I am not aware that any explanation has ever been given of this want of generality; or that any one has made known how it comes to pass, that while the developments of

* Communicated by the Author.

sin x, cos x, &c., in terms of x are true in all cases, yet the developments of the inverse functions, which, *à priori*, we should expect to possess equal generality, turn out to be true only in one particular case. In allusion to this circumstance Lacroix remarks: "Les series qui expriment sin x et cos x par l'arc, sont plus générales que leurs inverses, qui expriment l'arc par le sinus ou le cosinus: les dernières ne conduisent qu'au plus petit des arcs qui ont le même sinus ou le même cosinus, tandis que les premières donnent le sinus ou le cosinus, quel que soit celui de ces arcs qu'on prenne pour x."—*Lacroix*, *Calcul Diff. et Int.*, tom. iii. p. 620.

Now it is the object of this short paper to show that the series for $\sin^{-1}x$, $\cos^{-1}x$, &c., when properly investigated, possess the same generality as those for sin x, cos x, and that the defective forms above arise from an oversight committed in the analytical processes whence they are deduced.

If we turn to Lacroix, or to any other writer on the Calculus, we find the investigation of the development of $y = \sin^{-1}x$ conducted as follows:

$$y = \sin^{-1}x$$

$$\frac{dy}{dx} = (1-x^2)^{-\frac{1}{2}}$$

$$\frac{d^2y}{dx^2} = x(1-x^2)^{-\frac{3}{2}}$$

$$\frac{d^3y}{dx^3} = (1-x^2)^{-\frac{3}{2}} + 3x^2(-x^2)^{-\frac{5}{2}}$$

&c. &c.

The values of these expressions for $x = 0$ are said to be

$$y = 0, \ \frac{dy}{dx} = 1, \ \frac{d^2y}{dx^2} = 0, \ \frac{d^3y}{dx^3} = 1, \text{ \&c.};$$

and hence, by Maclaurin's theorem, the first of the above developments is inferred. Now when $x = 0$ it is not necessarily true that $y = 0$; it is true only when the proposed arc is less than a quadrant. If the arc be greater than a quadrant, and terminate in the second quadrant, then the value of it for $x = 0$ can obviously be no other than $y = \pi$; if it terminate in the fourth quadrant, its value for $x = 0$ must be $y = 2\pi$; if in the fifth, $y = 3\pi$, and so on*. It must also be observed that when the arc terminates in the second quadrant,

* We are here considering only the positive arcs; the negative arcs having the same sign will terminate in the 3rd, 4th, 7th, &c., quadrants, going round the circle in the opposite direction; and the corresponding values of y, for $x = 0$, will obviously be $-\pi$, -2π, -3π, &c., the same as before, but with opposite signs.

$\frac{dy}{dx}$, and therefore the following coefficients, are negative; when it terminates in the fourth quadrant they are positive, as at first; when in the fifth negative, and so on. Hence the general expression for the development of $\sin^{-1} x$, terminating in the $k+1$th quadrant, is

$$\sin^{-1} x = k\pi \pm \left\{ x + \frac{x^3}{1.2.3} + \frac{3^2 x^5}{1.2.3.4.5} + \frac{3^2 5^2 x^7}{1.2.3.4.5.6.7} + \&c. \right\}$$

the upper sign being used when $k+1$ is odd, and the lower when it is even, $k+1$ being either positive or negative.

The series for $y = \cos^{-1} x$ is inferred from the following conditions, which are said to have place when $x = 0$, viz.

$$y = \frac{\pi}{2},\ \frac{dy}{dx} = -1,\ \frac{d^2 y}{dx^2} = 0,\ \frac{d^3 y}{dx^3} = 1,\ \&c.$$

But the first of these conditions is true only when the proposed arc, y, terminates in the first quadrant. If it terminate in the fourth, y must be $\frac{3\pi}{2}$, when $x = 0$; if it terminate in the fifth, y must be $\frac{5\pi}{2}$; if in the eighth, y must be $\frac{7\pi}{2}$, and so on. Moreover, in the fourth quadrant $\frac{dy}{dx}$, $\frac{d^2 y}{dx^2}$, &c., have signs contrary to those which they have in the first; in the fifth the signs are the same as in the first; in the eighth opposite, and so on. Hence the general expression for the development of $\cos^{-1} x$, is

$$\cos^{-1} x = \frac{(2k+1) \pm 1}{2} \cdot \frac{\pi}{2} \mp \left\{ x - \frac{x^3}{1.2.3} + \frac{3 x^5}{1.2.3.4.5} - \&c. \right\}$$

the arc terminating in the $k+1$th quadrant; the upper sign being used when $k+1$ is odd, and the lower when it is even. It is obvious that if the arc be negative this expression will take the minus sign.

By attending to similar considerations we shall find, for the development of $\tan^{-1} x$, the general expression

$$\tan^{-1} x = k\pi + x - \frac{x^3}{3} + \frac{x^5}{5} - \frac{x^7}{7} +, \&c.,$$

the arc always terminating in the $2k+1$th quadrant.

The development of $\tan^{-1} x$ is frequently deduced by the aid of a certain logarithmic formula, without the application of the calculus; the resulting form is, however, limited to the single case above mentioned; and this limitation arises, as in the former process, from an oversight in the investigation, although one of a very different kind. The investigation we

have in view is as follows. Setting out from the known formulas—(See Young's Diff. Calc., p. 30.)

$$e^{x\sqrt{-1}} = \cos x + \sqrt{-1}.\sin x$$

$$e^{-x\sqrt{-1}} = \cos x - \sqrt{-1}.\sin x,$$

and taking the logarithms, we have

$$x\sqrt{-1} = \log(\cos x + \sqrt{-1}.\sin x)$$

$$-x\sqrt{-1} = \log(\cos x - \sqrt{-1}.\sin x);$$

therefore, by subtraction,

$$2x\sqrt{-1} = \log\frac{\cos x + \sqrt{-1}.\sin x}{\cos x - \sqrt{-1}.\sin x} = \log\frac{1+\sqrt{-1}.\tan x}{1-\sqrt{-1}.\tan x}.$$

$$\text{Now, } \log\frac{1+u}{1-u} = 2\left\{u + \frac{u^3}{3} + \frac{u^5}{5} + \frac{u^7}{7} +, \&\text{c.}\right\}$$

hence, substituting $\sqrt{-1}.\tan x$ for u, we have

$$2x\sqrt{-1} = 2\left\{\tan x - \frac{\tan^3 x}{3} + \frac{\tan^5 x}{5} - \frac{\tan^7 x}{7} +, \&\text{c.}\right\}\sqrt{-1}$$

$$\therefore x = \tan x - \frac{\tan^3 x}{3} + \frac{\tan^5 x}{5} + \frac{\tan^7 x}{7} +, \&\text{c.}$$

The reason that we have obtained this defective form arises from an omission in the general expression for the logarithm of $\frac{1+u}{1-u}$, which omission would lead to no error if we were dealing with *real* quantities only. It was proved by Euler, that every number has, besides the logarithm usually considered, an infinite number of others, all imaginary*: thus, if we represent the usually received value of log A by Log A, then will the general expression for log A be

$$\log A = \text{Log } A + 2k\pi\sqrt{-1}.$$

When, therefore, we are dealing with imaginary quantities, we are not warranted in omitting the imaginary values of log A, as is done in the foregoing process. Restoring, therefore, what has been improperly omitted, we have

$$\log\frac{1+u}{1-u} = 2\left\{u + \frac{u^3}{3} + \frac{u^5}{5} + \frac{u^7}{7} +, \&\text{c.}\right\} + 2k\pi\sqrt{-1}$$

---

* This may be proved as follows. By Young's Calculus, page 33,

$$\sqrt{-1} = e^{\frac{\pi}{2}\sqrt{-1}} \therefore -1 = e^{\pi\sqrt{-1}} \therefore 1 = e^{2k\pi\sqrt{-1}} \therefore A = Ae^{2k\pi\sqrt{-1}}$$

$\therefore \log A = \log A + 2k\pi\sqrt{-1}.$

$$\therefore\ 2x\sqrt{-1} = 2\left\{\tan x - \frac{\tan^3 x}{3} + \frac{\tan^5 x}{5} - \&c.\right\}\sqrt{-1}$$
$$+ 2k\pi\sqrt{-1}$$

$$\therefore\ x = \tan x - \frac{\tan^3 x}{3} + \frac{\tan^5 x}{5} -, \&c., + k\pi$$

as before obtained.

Many other developments, besides this, are deduced in an incomplete form from neglecting imaginary logarithms. Take the following from among many that might be selected. Lacroix, at p. 136, vol. i. of his Calculus, develops the expression $(\sqrt{-1})^{\sqrt{-1}}$ as follows:

Substitute $\sqrt{-1}$ for u in the known formula

$$\log u = u - u^{-1} - \tfrac{1}{2}(u^2 - u^{-2}) + \tfrac{1}{3}(u^3 - u^{-3}) -, \&c.,$$

and it becomes

$$\log\sqrt{-1} = \sqrt{-1} - \frac{1}{\sqrt{-1}} - \frac{1}{2}(-1+1) + \frac{1}{3}\Big(-\sqrt{-1}$$
$$+ \frac{1}{\sqrt{-1}}\Big) -, \&c.$$

$$= \frac{-2}{\sqrt{-1}}\left\{1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} +, \&c.\right\}$$

$$= \frac{-2}{\sqrt{-1}} \cdot \frac{\pi}{4} = \frac{\pi}{2}\sqrt{-1}$$

$$\therefore\ \sqrt{-1}\log\sqrt{-1} = -\frac{\pi}{2}\ \therefore (\sqrt{-1})^{\sqrt{-1}} = e^{-\frac{\pi}{2}}.$$

This result is incomplete, for it is known that

$$(\sqrt{-1})^{\sqrt{-1}} = e^{-\frac{4k+1}{2}\pi};$$

and this is the result which we should have obtained if the imaginary quantity $2k\pi\sqrt{-1}$ had not been improperly omitted; for we should then have had

$$\log\sqrt{-1} = \frac{\pi}{2}\sqrt{-1} + 2k\pi\sqrt{-1}$$

$$\therefore\ \sqrt{-1}\log\sqrt{-1} = -\frac{\pi}{2} - 2k\pi = -\frac{4k+1}{2}\pi.$$

$$\therefore\ (\sqrt{-1})^{\sqrt{-1}} = e^{-\frac{4k+1}{2}\pi}.$$

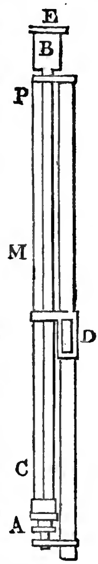
These instances not only verify Euler's theory of imaginary

logarithms, but at the same time show their use in analytical investigations.

July 1834. J. R. Young.

XXXI. *Description of an Improvement in the Construction of* Say's *Instrument for measuring Specific Gravities.* *By* W. H. M.*

A DESCRIPTION of this instrument as originally constructed, and which might with more propriety be called an instrument for measuring volumes, is given by its inventor, M. Say, a French officer of engineers, in one of the volumes of the *Annales de Chimie* for 1797 (xxiii, i.), and also by Mr. Faraday in his work on Chemical Manipulation. The annexed figure represents the instrument in its improved form. AB are two glass tubes, of 0·2 inch internal diameter, one 34 and the other 35 inches long, placed close together, and having their lower ends cemented into an iron cap, into the lower end of which an iron stop-cock is screwed. The upper end of the longer tube is cemented into the bottom of a cup B, having its rim ground truly plane, the capacity of which is a little more than half that of the tube. The tubes are fixed parallel to a graduated scale, carrying a sliding vernier D, provided with an index formed of two slips of brass, one before and the other behind the tubes, having their lower edges in a plane perpendicular to the scale. E is a piece of plate-glass, having its lower surface greased, and large enough to close the mouth of the cup B. The instrument may either be fixed permanently in a vertical position against a wall, like a barometer, or else may have a broad foot with three screws, by which it may be rendered vertical for use.



The substance to be examined, which may be any solid liquid or powder that is not volatile, is placed in a small cup, which goes into the cup B. The stop-cock at A is closed, and mercury is poured through a small funnel into the shorter tube till it rises to a mark on the longer tube at P; the mouth of the cup B is then closed, so as to be air-tight, by the plate of glass E. The stop-cock must now be opened, and the mercury permitted to escape in a small stream till its surface in the longer tube stands about 15 inches higher than in the shorter tube, when the stop-cock is to be closed.

* Communicated by the Author.

Lastly, the depths of M and C, the extremities of the columns of mercury in the tubes, below the mark at P, are to be measured by means of the scale and sliding-index.

Let v be the volume of the substance examined, u the space occupied by the air between E and P before the substance is placed in the cup, h the atmospheric pressure expressed in the height of the column of mercury, of the same temperature as the mercury in AB, which it supports.

At the commencement of the observation, when the surface of the mercury was at P and the cup B closed, the air between E and P occupied a space $= u - v$, and its pressure was measured by h; when the extremities of the columns of mercury in the two tubes are at M and C, the air which was in B occupies a space $= u - v + \text{vol. PM}$, and its pressure is measured by $h - \text{MC}$.

Hence, by Hooke's law,

$$\frac{u - v + \text{vol. PM}}{u - v} = \frac{h}{h - \text{MC}},$$

$$\therefore\ v = u - \frac{h - \text{MC}}{\text{MC}}\ (\text{vol. PM}).$$

When the bore of the longer tube is very uniform, and the area of a perpendicular section of it $=$ K, we have

$$v = u - \frac{h - \text{MC}}{\text{MC}}\ \text{K}\,.\,\text{PM}.$$

The value of u may be found by a similar process, the cup being empty. K is readily deduced from the weight of a column of mercury of known length contained in the longer tube.

If the weight of the substance be also known, its specific gravity can of course be easily calculated.

The advantages which the instrument here described possesses over that of M. Say, appear to be: (1.) It is only half the length of Say's, and requires no complicated framework for its support. (2.) The heights of the columns of mercury may be measured with great accuracy with the vernier. In Say's, the fractions of a division on the scale can only be obtained by estimation. (3.) By making both tubes of the same diameter, the effect of capillary depression is completely eliminated; while in Say's its amount, more especially in the space between the tubes, can never be accurately allowed for.

W. H. M.

XXXII. *Phytological Errors and Admonitions.* By the Rev. Patrick Keith, *F.L.S.**

THE profound research, the acute discrimination, and the eloquent expression of facts, of arguments, or of opinions displayed in the writings of M. De Candolle, have obtained for their author a high and well-merited reputation, whether as a botanist or phytologist; so much so, that we shall not err if we apply to him the well-known maxim of Horace, which says,

> .... Cui lecta potenter erit res,
> Nec facundia deseret hunc, nec lucidus ordo.
>
> *De Art. Poet.* 40.

I had been repeatedly led to make this application of the maxim in perusing different portions of his *Physiologie Végétale*; but especially in a late perusal of the *Considérations Préliminaires* which he affixes to that work, and in which he presents to the notice of the reader a succinct and correct view of the true principles of phytological investigation, marking specifically the path to be pursued and the errors to be avoided. The structure of the several organs of vegetables, as in a state of rest, is the first branch of the study of the phytologist—that is, the anatomy of the plant; their agency in the œconomy of vegetation is the second—that is, the study of the forces with which the organs act, or by which they are acted upon. In the former case we are to explore with the most rigid scrupulosity their hidden and internal structure; in the latter, we are not to ascribe to the vital energies of the plant effects that are merely tissual on the one hand, or chemical on the other; nor to its tissual or chemical properties, effects that are merely vital; but we are to be careful to assign to every effect its true and proper cause†.

Here we have, doubtless, the golden rule of all phytological investigation. Yet the investigator can make use of it only according to the best of his ability; and in the difficulty of accounting satisfactorily for the various phænomena of vegetation, it is not surprising that phytologists should have been occasionally seduced into error, and led to assign effects to wrong causes. The grand stone of stumbling is to be met with in accounting for those effects which are evidently vital, and in which the life of the vegetable seems to approximate closely to that of the animal. This is the respect that has led phytologists to overstep the limits which bound the legitimate scope of vegetable investigation, and to ascribe to vegetables faculties, the existence of which is at best but extremely dubious. Tissual susceptibilities did not seem to them sufficient

* Communicated by the Author.

† *Phys. Végét.*, tom. i. p. 6.

to account for certain phænomena of vegetable life, and hence they assumed the aid of susceptibilities of a higher order,—susceptibilities which in the animal subject are termed cerebral. Yet the leading advocates of this opinion have not been novices; they have not been men of mean reputation, whether in literature or in science; they did not take up the doctrine at random; they took it up deliberately and advisedly, and hence their opinions are entitled to our respectful consideration. Among them we find the names of Bonnet, Watson, Percival, Darwin, Smith. We do not say that M. De Candolle's notice of them is not respectful; but he regards the doctrine as supported more by arguments drawn from imagination and feeling, than from reason and experience, that is, from the heart rather than from the head*; and whether this implies anything of a compliment, we leave the reader to determine.

I find that my name is associated, in a foot note, with that of Bonnet, Percival, and Smith, not for having defended the doctrine of vegetable sensibility, I presume, for that I evidently controverted, but for having mooted the question in my System of Physiological Botany, and for maintaining, or seeming to maintain, a doctrine that may be said to imply sensation, namely, that of a vegetable instinct. At a loss to account for the singular, and hitherto inexplicable fact of the irresistible descent of the radicle, in the process of germination, through the intervention of any cause, whether chemical or mechanical merely, I thought it was not absurd to suppose in the vegetable subject, the existence and agency of a cause analogous to that of instinct in the animal subject, but not identical with it†. But although a zoologist is allowed the aid of instinct to account for the *unaccountables* of the animal œconomy, yet let me admonish the phytologist who has any respect for the opinion of the critics, to beware how he lays claim to the aid of an analogous principle to account for the *unaccountables* of the vegetable œconomy. He will be set down immediately as a fool or a blockhead. His case is without remedy, and his sentence without mercy. He will be told that the doctrine "is palpably inadmissible;" that "it is absurd;" and that "it is merely a betraying of his ignorance of the cause." Did Newton know the cause which he points out to our notice by the term gravitation?

* *Phys. Végét.*, tom. i. p. 29.

† [Agreeing entirely with Mr. Keith in this view of the subject, we may add, further, (adopting Macleay's distinction of relations of analogy from those of affinity, and employing a new word much wanted,) that this vegetable principle, though strictly *analogous* to instinct, is not merely not *identical* but not even *affinal* with it.—E. W. B.]

He knew merely the laws by which a certain principle acts, and he gave it a name. Beyond that he knew nothing. The same is the case with the physiologist who talks of a vital principle, and with the phytologist who ascribes the descent of the radicle to a cause analogous to that of animal instinct. He knows very well that he is ignorant of the cause any further than as producing effects that can result only from the exertion of the vital energies of the plant; and to the energies thus exerted he gives the name of instinct. The best and most acute of our phytologists have felt the strong leaning that exists in the human mind to the admission of some such principle. What says Dutrochet in his *Observations sur la Motilité des Végétaux*? "En voyant cette diversité de moyens employés pour parvenir à un même fin, on serait presque tenté de croire qu'il existe la une intelligence secrète qui choisit les moyens les plus convenables pour accomplir une action determinée." (p. 132.)

For myself, I have abandoned the use of the term instinct entirely; not because the admission of vegetable instinct is a confession of our ignorance of the cause,—for it is better to confess our ignorance of a cause than to adduce a false one,—but because it is a term calculated to mislead the phytological student, as being so long usurped by the zoologist, and a term, the legitimate use of which seems to me to be that of the denoting of the act of an individual being regarded as sentient, rather than that of the denoting of the growth of any organ or fabric, whether animal or vegetable. Hence, as I disclaim the doctrine of vegetable sensation *, I disclaim, of necessity, that of vegetable instinct also.

Still I cannot adopt the opinion of those who ascribe the descent of the radicle merely to the force of gravitation. To me the phænomenon seems to be essentially a vital process, which the phytologist is not to confound with any process that is essentially either chemical or mechanical. This is an error, against the commission of which M. De Candolle is at great pains to guard his reader in his *Considérations Préliminaires*†, and yet, after all, he no sooner finishes his admonitions than he seems to me to fall into it himself; for he announces his adoption of Mr. Knight's theory of the descent of the radicle,—that is, the accounting for a vital phænomenon upon a principle merely mechanical‡.

It is not my intention to enter into the merits of Mr. Knight's hypothesis at present: a more convenient season may arrive. What I have to remark now is, that M. De Candolle, on what

* Phil. Mag. and Annals, N.S., April 1832.

† *Phys. Végét.*, tom. i. p. 7.

‡ *Ibid.*, tom. i. p. 30; ii. p. 821.

ground I know not, unless as involved in the theory of Mr. Knight, adopts an additional opinion, which I cannot help regarding as erroneous also. It is the opinion by which vegetating roots are supposed to receive their increments of length solely by the extreme points. "Les racines ne s'alongent que par leur extremité*." That this was the result of some experiments of Duhamel and of Mr. Knight, we do not mean to deny; but it is good only as far as it goes. You are not to deduce from it a rule for all plants whatever. If you do so, you embrace a manifest error which I did my best to expose, so long ago as 1819, in a set of experiments published in No. 76 of Dr. Thomson's Annals of Philosophy. Yet I do not find that any one ever condescended to notice them, or to embrace the truth which they establish on account of them. Truth, lovely though she be, if she hopes to be well received, must come with a recommendation in her right hand, that is, a recommendation from some great and acknowledged authority; for there are many sciolists who would rather be in the wrong with a writer of high reputation than in the right with one of minor celebrity. Yet I find that Mr. Lindley has at last taken up the subject, and exposed the erroneousness of the old opinion†, so that from henceforward it is to be believed that botanists will no longer be found to cling to this palpable error. "On the 5th of August," says Mr. Lindley, "I tied threads tightly round the root of a Vanilla, so that it was divided into three spaces, of which one was 7 inches long, another 4 inches, and a third, which was the free growing extremity, 1 inch and $\frac{3}{8}$ths. On the 19th of September the first space measured $7\frac{1}{8}$ inches, the second $4\frac{3}{8}$ inches, and the third, or growing, $2\frac{1}{8}$ inches." A root of *Aerides cornutum* gave a similar result, and established the fact of the general elongation of the root in the plants specified. Yet Mr. Lindley gives all the weight that is due, or perhaps even more than is due, to the experiments of Duhamel and Mr. Knight; for he thinks it is possible that the peculiarity of elongation by the extreme points may be universal in exogegenous plants. But the experiments which I instituted on the radicle of the bean show that it is not.

"On the first of October 1818, I sowed some tick beans in a small earthen pan filled with garden mould.

"On the fourth the radicle of the most forward had protruded about $\frac{1}{5}$th of an inch beyond the integuments, when I marked it with ink at the point, in the middle, and at the base, as clearing the integuments; so that the marks were about $\frac{1}{10}$th of an inch from each other.

* *Phys. Végét.*, tom. i. p. 41; ii. p. 822. † Introd., p. 228.

"On the fifth the radicle was $\frac{1}{4}$th of an inch in length, and the marks nearly as before with regard to their relative distances, but removed evidently from the integuments, so as to admit a fourth or additional mark adjoining the integuments. The radicle, which was originally upright, was now bending down.

"On the sixth the radicle was $\frac{1}{2}$ an inch in length, the first mark being within two or three lines of the point; the second at about $\frac{1}{6}$th of an inch from the first; the third at about $\frac{1}{6}$th of an inch from the second; and the fourth at about $\frac{1}{8}$th of an inch from the third, as well as perceptibly removed from the integuments.

"On the seventh the radicle was $\frac{3}{4}$ths of an inch in length. The first mark was still within two or three lines of the point; the second was at the distance of $\frac{1}{4}$th of an inch from the first; the third was at the distance of $\frac{1}{4}$th of an inch from the second; and the fourth was at about $\frac{1}{8}$th of an inch from the third, being but little more than its original distance, but removed to the distance of $\frac{1}{8}$th of an inch from the integuments.

"On the eighth the radicle was one inch in length, the first mark being still near the apex; the second at the distance of about $\frac{1}{3}$rd of an inch from the first; the third at the distance of $\frac{1}{3}$rd of an inch from the second; and the fourth nearly as before.

"On the ninth the radicle was $1\frac{1}{4}$ inch in length, the three marks next the base being nearly as before, and the mark next the apex being the only one that was carried down.

"On the tenth, and as long as any further observations were made, it was still the lower extremity of the radicle that was carried down. But enough had been previously observed to show that the assumed peculiarity of the elongation of the radicle is founded in a mistake; and that the root in its incipient state, like the stem in *its* incipient state, is augmented by the introsusception and deposition of additional particles throughout its whole mass, or 'by a general extension of parts already formed,' though it may afterwards, like the stem, become so firm and compact as no longer to admit of augmentation in that way*."

As connected with the presumed mode of the root's augmentation, M. De Candolle takes occasion to speak of the spongioles in which the small fibrils of the root terminate. He represents them as being composed essentially of a cellular tissue. "Les spongioles des racines sont essentiellement composées de tissu cellulaire†." Yet this account of their internal structure does not seem to me to accord well with the

* Annals of Phil., No. 76. † *Phys. Végét.*, tom. i. pp. 71, 41.

facts of the case, at least as they have presented themselves to my inspection. To me the spongioles seemed to present the palpable rudiments of a vascular tissue also.

It had been asserted by Duhamel and others, that the extreme fibrils of the roots of plants die annually, in the winter, and are renovated in the spring, in the manner, it may be presumed, of leaves. Mr. Knight admitted the position with regard to bulbous roots, but denied it with regard to the roots of trees. To satisfy myself on this point, I took up portions of the roots of several different plants, trees chiefly, which brought the structure of the spongioles unavoidably under my notice.

On the first of September 1828, I took up a portion of the root of a plant of the horse-chestnut, which had come up from seed in the preceding spring. The fibrils, which were for the most part in a vigorous state, presented the appearance of a cylindrical mass of pulp, of a whitish colour, inclosing a firm and central filament, and terminating ultimately in an enlarged and flattened point,—the spongiole of De Candolle,—which, when cut open, was found, as well as the rest of the fibril, to inclose a fine and central filament, or rather a bundle of such filaments united into one cord, like the filaments that compose the nerves of animals. A few had begun to decay, and their decay was indicated by the shrinking and shrivelling up of the external and pulpy mass. But if some were thus in decay, others were beginning to shoot out fresh and vigorous from the larger divisions of the *chevelure*, so that there was no appearance of the commencement of any regular denudation of the root, whether by the decay of fibril or of spongiole.

On the 17th I took up the whole plant, and the appearances were as before.

On the 15th of November I took up a plant of dwarf box, but did not find a single fibril in decay. The newly formed ones were covered with a white, dense, and fine down throughout the greater part of their length, but at the origin and point they were smooth. They were also more tapering than those of the horse-chestnut, and had less of the club-shaped appearance.

On the 28th I took up part of the root of an apricot, but found no fibril wholly in decay. The outer coat of some of them was indeed in decay, but the central filament was still full of moisture, and a new coat forming.

On the first of December I had some portions of the root of an elm-tree taken up, at about ten or twelve feet from the caudex. They were well furnished with fibrils, many of which were in decay, but their loss was more than compen-

sated by a new and vigorous progeny, protruding, like little buds, from the surface of the larger divisions. Like those of the horse-chestnut, they terminated ultimately in a soft bibulous and club-shaped appendage, or spongiole, furnished with a central filament, that seemed to inclose others still smaller. The external and pulpy coat of the spongiole was often found to be in decay, while the inclosed and central filament was still quite sound.

From the above observations respecting roots, the two following inferences are obviously deducible.

I. The root is never wholly denuded of its fibrils, or spongioles, as the branches are denuded of their leaves. A partial decay, with a partial renovation of these organs, seems to be occurring at all seasons; but a total denudation of the root occurs at no season. If, with Mr. Knight, we admit of a total denudation of bulbous roots, I think it will not go beyond such as are taken up out of the soil for the winter; for if the bulb is allowed to remain in the earth, it is to be believed that new fibrils will have begun to be protruded before the old ones have finally decayed.

II. Spongioles cannot be said to be very well described by representing them as composed merely of a cellular tissue, for surely the central filament is to be regarded as exhibiting the rudiments of a vascular tissue also; and if in some cases it is so very fine and minute as to be scarcely perceptible, still it is abundantly perceptible in the cases now specified. I regret, however, that I have not had access to a copy of M. De Candolle's *Organographie*, for there, I doubt not, the structure of spongioles is more minutely detailed, and in such a way, perhaps, as might have precluded the necessity for any particular remark. What I have said is founded on such representations of the structure of the spongiole as are to be met with in his *Physiologie Végétale*.

Charing, Kent, Aug. 1st, 1834. P. KEITH.

XXXIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.*

1834. Feb. 26.—A PAPER "On the Quantity of Solid Matter suspended in the Water of the Rhine," by Leonard Horner, Esq., F.G.S., F.R.S., was first read.

The experiments referred to in this paper, were made by the author at Bonn, in the months of August and November. The apparatus which he used was a stone bottle capable of containing about a gallon, and furnished with a cork covered with greased

* Continued from p. 70.

leather, having a long string attached to it. A weight was suspended from the bottle by a rope of such a length, that when the weight touched the ground, the mouth of the bottle was at the desired distance from the bottom of the river. The cork was then removed by the string, and the instant the bottle was full it was drawn up.

The first set of experiments was made in August, at 165 feet from the left bank of the river, and at 7 feet from the surface, or 6 feet from the bottom. The Rhine was unusually low, and the water was turbid and of a yellowish colour. The quantity of solid matter obtained from a cubic foot of water, and slowly dried, was 21·10 grains, or about $\frac{1}{20,734}$th part. The residuum effervesced briskly with diluted muriatic acid, was of a pale yellowish-brown colour, smooth to the touch, and in appearance and properties undistinguishable from the loess of the Rhine Valley.

The second set of experiments was made in November on water taken from the middle of the river, and about one foot below the surface. A great deal of rain had fallen some time before, and also fell during the experiment. The water was of a deeper yellow than on the former occasion, but when taken up in a glass was not very different in appearance. The residuum of a cubic foot weighed 35 grains, or the $\frac{1}{12,500}$th part. The author then enters into an approximate calculation of the medium quantity of earthy matter borne down by the Rhine during 24 hours. He assumes that the annual mean breadth of the river opposite Bonn, is 1,200 feet, the mean depth 15 feet, the mean velocity $2\frac{1}{2}$ miles in an hour, and the average amount of solid matter held in suspension to be 28 grains in the cubic foot of water. From these data he deduces the result that 145,981 cubic feet of solid matter are borne past Bonn every 24 hours.

A paper was next read, entitled, "Observations on the Geological Structure of the Neighbourhood of Reading," by J. Rofe, Esq., and communicated by Robert Hunter, Esq., F.G.S.

This communication was accompanied by a collection of fossils from the neighbourhood of Reading, and was prepared by the author chiefly to point out the localities and geological connexions of the specimens; but he also describes some beds belonging to the plastic clay, which are not mentioned in the published accounts of the district.

In detailing the section presented by the Katsgrove pits, he states, that the upper part of the chalk is perforated to about the depth of a foot by tubular cavities resembling those made by the Teredo in wood. The oyster-bed, which rests upon the chalk, he says, is divisible into two parts, each about a foot thick, the lower consisting principally of brown clay, and the upper of the sand containing green particles. Above this bed the author observed about a foot and a half of clay, and in the quartzose sand resting upon it a layer of ochreous nodules. The account given by Dr. Buckland of the strata above the one last mentioned*, the author says is correct, with the exception of a thin bed of shells which

* Geological Transactions, 1st series, vol. iv. p. 278.

occurs about 6 feet from the top of the section, and which is not mentioned in Dr. Buckland's paper. This bed was also noticed by Mr. Rofe in many other places in the immediate neighbourhood of Reading, and at Woodley Lodge, about three miles to the east of the town. At the latter locality the order of the beds is as follows:

Blue clay, about	40 feet.
The shelly stratum.	
Mottled clay..............	55 —
Ditto, occasionally sandy....	35 —

In conclusion, the author states that all the wells in Reading, excepting those supplied by land-springs, both on the north and south of the Kennet, and even within 30 yards of its banks, are regulated by the Thames, rising and falling with that river. This phænomenon, he conceives, may be accounted for, by the Kennet flowing over a bed of tenacious clay, whereas the Thames flows over gravel resting immediately upon chalk, into which the wells are sunk.

March 12.—A paper was read, entitled, "Observations on the Temple of Serapis at Pozzuoli, near Naples; with Remarks on certain Causes which may produce Geological Cycles of great Extent. In a Letter to W. H. Fitton, M.D., from Charles Babbage, Esq."

The author commences this paper with a general description of the present state of the Temple of Serapis, and gives the measurement of the three marble columns which remain standing, and which, from the height of 11 feet to that of 19, are perforated on all sides by the *Modiola lithophaga* (of Lamarck); the shells of that animal remaining in the holes formed by them in the columns. A description is then given of the present state of twenty-seven portions of columns, and other fragments of marble, and also of the several incrustations formed on the walls and columns of the temple.

The conclusions at which the author arrives are—

1. That the temple was originally built, at or nearly at the level of the sea, for the convenience of sea-baths, as well as for the use of the hot spring which still exists on the land side of the temple.

2. That at some subsequent period the ground on which the temple stood subsided slowly and gradually; the salt water, entering through a channel which connected the temple with the sea, or by infiltration through the sand, mixed itself with the water of the hot spring containing carbonate of lime, and formed a lake of brackish water in the area of the temple, which, as the land subsided, became deeper, and formed a dark incrustation.

The proofs are, that sea-water alone does not produce a similar incrustation; and that the water of the hot spring *alone* produces an incrustation of a different kind; also, that Serpulæ are found adhering to this dark incrustation; and that there are lines of *water-level* at various heights, from 2·9 feet to 4·6 feet.

3. The area of the temple was now filled up to the height of about seven feet with ashes, tufa, or sand, which stopped up the

channel by which sea-water had been admitted. The waters of the hot spring thus confined converted the area of the temple into a lake, from which an incrustation of carbonate of lime was deposited on the columns and walls.

The proofs are, that the lower boundary of this incrustation is irregular; whilst the upper is a line of water-level, and that there are many such lines at different heights;—that salt water has not been found to produce a similar incrustation;—that the water of the Piscina Mirabile, which is distant from the sea, but in this immediate neighbourhood, produces, according to an examination by Mr. Faraday, a deposit almost precisely similar;—that no remains of Serpulæ, or other marine animals, are found adhering to it.

4. The temple continuing to subside, its area was again partially filled with solid materials; and at this period it appears to have been subjected to a *violent* incursion of the sea. The hot-water lake was filled up, and a new bottom produced, entirely covering the former bottom, and concealing also the incrustation of carbonate of lime.

The proofs are, that the remaining walls of the temple are highest on the inland side, and decrease in height towards the sea side, where they are lowest;—that the lower boundary of the space perforated by the marine Lithophagi is, on different columns, at different distances beneath the uppermost or water-level line;—that several fragments of columns are perforated at the ends.

5. The land continuing to subside, the accumulations at the bottom of the temple were submerged, and Modiolæ attaching themselves to the columns and fragments of marble, pierced them in all directions. The subsidence continued until the pavement of the temple was at least nineteen feet below the level of the sea.

The proofs are derived from the condition of the columns and fragments.

6. The ground on which the temple stood appears now to have been stationary for some time, but it then began to rise. A fresh deposition, of tufa or of sand, was lodged, for the third time, within its area,—leaving only the upper part of three large columns visible above it.

Whether this took place before or subsequently to the rise of the temple to its present level, does not appear; but the pavement of the area is at present *level* with the waters of the Mediterranean.

The author then states several facts, which prove that considerable alterations in the relative level of the land and sea have taken place in the immediate vicinity. An ancient sea-beach exists near Monte Nuovo, two feet above the present beach of the Mediterranean;—the broken columns of the Temples of the Nymphs and of Neptune, remain at present standing *in the sea*;—a line of perforations of Modiolæ, and other indications of a water-level 4 feet above the present sea, is observable on the sixth pier of the bridge of Caligula; and again on the twelfth pier, at the height of

10 feet ;—a line of perforations by Modiolæ is visible in a cliff opposite the island of Nisida, 32 feet above the present level of the Mediterranean.

The author considers the preceding inferences as a legitimate induction from the observed and recorded facts; and proceeds to suggest an explanation of the gradual sinking and subsequent elevation of the ground on which the temple stands. From some experiments of Col. Totten, recorded in Silliman's Journal, he has calculated a table of the expansion, in feet and decimal parts, of granite, marble, and sandstone, of various thicknesses, from 1 to 500 miles, and produced by variations of temperature of 1°, 20°, 50°, 100°, 500° of Fahrenheit: and he finds from this table, that if the strata below the temple expand equally with sandstone, and a thickness of five miles were to receive an accession of heat equal only to 100°, the temple would be raised 25 feet;—a greater alteration of level than is required to account for the phænomena in question. An additional temperature of 50° would produce the same effect upon a thickness of ten miles; and an addition of 500° would produce it on a bed only a single mile in thickness.

Mr. Babbage then adverts to the various sources of volcanic heat in the immediate neighbourhood: and he conceives that the change of level may be accounted for by supposing the temple to have been built upon the surface of matter at a high temperature, which subsequently contracted by slowly cooling down ;—that when this contraction had reached a certain point, a fresh accession of heat from some neighbouring volcano, by raising the temperature of the beds again, produced a renewed expansion, and which restored the temple to its present level. The periods at which these events happened are then compared with various historic records.

The second part of this letter contains some views, respecting the possible action of existing causes, in elevating continents and mountain-ranges, which occurred to the author in reflecting on the preceding explanation. He assumes as the basis of this reasoning the following established facts:

1. That as we descend below the surface of the earth at any point, the temperature increases.
2. That solid rocks expand by being heated; but that clay and some other substances contract under the same circumstance.
3. That different rocks and strata conduct heat differently.
4. That the earth radiates heat differently, or at different parts of its surface, according as it is covered with forests, with mountains, with deserts, or with water.
5. That existing atmospheric agents and other causes, are constantly changing the condition of the surface of the globe.

Mr. Babbage then proceeds to remark, that whenever a sea or lake is filled up, by the continual wearing down of the adjacent lands, new beds of matter, conducting heat much less quickly than water carries it, are formed; and that the radiation, also, from the surface of the new land, will be different from that from the water. Hence, any source of heat, whether partial or central, which pre-

viously existed below that sea, must heat the strata underneath its bottom, because they are now protected by a bad conductor. The consequence must be, that they will raise, by their expansion, the newly formed beds above their former level;—and thus the bottom of an ocean may become a continent. The whole expansion, however, resulting from the altered circumstances, may not take place until *long* after the filling up of the sea; in which case its conversion into dry land will result partly from the filling up by detritus, and partly from the rise of the bottom. As the heat now penetrates the newly formed strata, a different action may take place; the beds of clay or sand may become consolidated, and may contract instead of expanding. In this case, either large depressions will occur within the limits of the new continent, or, after another interval, the new land may again subside, and form a shallow sea. This sea may be again filled up by a repetition of the same processes as before:—and thus alternations of marine and freshwater deposits may occur, having interposed between them the productions of dry land.

Mr. Babbage's theory, therefore, may be thus briefly stated.—In consequence of the changes actually going on at the earth's surface, the *surfaces* of equal temperature within its crust, must be continually changing their form, and exposing thick beds, near the exterior, to alterations of temperature; the expansion and contraction of these strata will probably form rents, raise mountain-chains, and elevate even continents.—The author admits that this is an hypothesis; but he throws it out, that it may be submitted to an examination which may refute it if fallacious,—or, if it be correct, establish its truth,—because he thinks that it is deduced directly from received principles, and that it promises an explanation of the vast cycles presented by the phænomena of geology.

March 26.—A letter was first read from Charles Denham Orlando Jephson, Esq., M.P., F.G.S., to George Bellas Greenough, Esq., P.G.S., "On Variations of Temperature in a Thermal Spring at Mallow."

The observations recorded in this letter were made principally during the autumn and winter months of 1833. The extreme variations were 67° and $71°\frac{3}{10}$, the difference depending in Mr. Jephson's opinion on the quantity of water acted upon. From 80 to 100 yards north of the spring at which the observations were made, are other thermal springs, the temperature of which is 1° higher, and from 60 to 80 yards to the south is a cold spring, having a temperature of 54°. The formation from which the waters issue is limestone.

A letter was then read from William Henry Egerton, Esq., F.G.S., addressed to Charles Lyell, Esq., For. Sec. G.S., "On the Delta of Kander."

The Kander in its ancient course flowed parallel to the Lake of Thun, and emptied itself into the Aar, beyond the village of Heimberg; but in consequence of the injury done to the land by

its frequent inundations, the Government of Bern determined to direct its waters into the lake of Thun. This object was finally accomplished about the year 1713, by making two parallel tunnels, about a mile in length, between the original course of the river and the lake; and no sooner was the Kander admitted into them than it burst up the arches, tore away the masses of rock which obstructed its passage, and bore a vast heap of gravel and detritus into the lake. The delta thus commenced, and increased by the sedimentary matter brought down during nearly 120 years, now presents a tract covered with trees, extending about a mile along the original shore, and a quarter of a mile from it into the lake. The depth of the ravine by which the Kander now enters the lake is 50 feet. The depth of water at the part occupied by the delta Mr. Egerton could not ascertain, but, from the declivity of the ancient banks, he conceives that it must have been considerable, and Saussure found some parts of the lake to be 350 French feet in depth. The author determined by actual measurement the angle at which the new deposit dips beneath the waters of the lake, at the extremity of the delta. At 30 yards from the shore, he found 14 fathoms of water; at 60 yards, 23 fathoms, and at 120 yards no bottom at a depth of 32 fathoms.

A communication by Colonel Sykes was then read, entitled, "A Notice respecting some Fossils collected in Cutch by Captain Smee, of the Bombay Army."

The district from which the specimens were procured is situated between the 23rd and 24th parallels of N. lat. and 70th and 71st degrees of E. long., and is bounded on the E. and S. by the Run. The fossils consist of four species of Ammonites, one of Trigonia, two of Astarte, one of Corbula, one of Gryphæa, and a coral having a nummulitic form. One of the Ammonites has a general resemblance to *A. Wallichii*, and another agrees in some respects with *A. Nepalensis*, both of which occur in the fossils procured in the Himalaya range; one specimen of the Gryphæa is stated to resemble closely the Gryphæa of the Oxford clay of England; and the coral belongs to the genus which occurs in the Kressenberg iron ore. Specimens of silicified wood, lignite and alabaster from Cutch were also exhibited, and of durable oolite from near Poorbunda, on the W. coast of the peninsula of Goojrat; and it is stated that the same rock is found abundantly at Raujcote in the centre of the peninsula. In conclusion, Colonel Sykes observes, that if English analogies may be taken for a guide, the district from which the above fossils were obtained would seem to be composed of secondary rocks, as Ammonites, Trigonia, and Gryphæa have not been found in England higher than the upper chalk.

A paper was afterwards read "On the Gravel and Alluvial Deposits of those Parts of the Counties of Hereford, Salop, and Worcester which consist of Old Red Sandstone; with an Account of the Puffstone, or Travertin of Spouthouse, and of the Southstone Roch near Tenbury, by Roderick Impey Murchison, Esq., V.P.G.S.

The district of which the transported materials are described is bounded on the west by the transition rocks, extending from the environs of Ludlow to the S.W. of Kington, and on the E. by the Abberley Hills, thus including a large portion of the trough of old red sandstone. It is shown that all the detritus within these limits has been derived from the adjacent rocks. In the neighbourhood of Kington bowlders of the contiguous trap rocks are found upon the talus of the Ludlow rocks, extending to the edge of the old red sandstone, and in the plain W. of Ludlow the surface of the old red is frequently covered with coarse gravel, in which are numerous fragments of the trap rocks of the W. of Shropshire. In general, however, the gravel is chiefly made up of the debris of transition rocks, the coarser varieties being found only near the boundary of those formations. Detailed sections are given in the neighbourhood of Ledbury; and the valley of the Teeme is particularly described to prove that, on receding from the transition chains, the gravel becomes more finely comminuted, and exhibits near Tenbury the characters of lacustrine or fluviatile sediments, the materials of which consist exclusively of fragments of the surrounding rocks of old red sandstone.

Two remarkable cases of a modern travertin, 5 and 8 miles E. of Tenbury, are then cited, the one near the Spouthouse farm, the other the Southstone Roch, both of which have been accumulated in narrow dells, which intersect transversely promontories of the old red sandstone. At the former the travertin is associated with much sandy marl. The latter is a cavernous rock of about 50 feet in height, and has a superficies of more than a quarter of an acre, having on its surface a small house and garden. In both cases the travertin incloses Helices of existing species, and has been occasionally quarried for purposes of building and burning to lime.

These modern rocks are shown to have been formed by small springs which issue from the calcareous or cornstone strata of old red sandstone, and still encrust the leaves and grasses over which they flow, a process which the author (judging from the size of the rocks produced) supposes to have been in undisturbed action during the whole period of history.

Although no bowlders of foreign rocks are to be found in the districts above described, it is stated that on the northern confines of the old red sandstone near Bridgenorth and Wenlock, there are many large fragments of granite of various sorts. These are also seen in abundance on the flanks of the Wrekin, but they are not to be traced into the area of the old red sandstone, and as they are entirely different from any of the Welsh rocks, the author refers them to a northern region. In conclusion, it is suggested that the superficial accumulation of the old red sandstone of Salop, Herefordshire, and Worcestershire may be referred to causes in operation during three epochs, which may hereafter be divided into other distinct periods.

1. To the currents caused by the elevation of the adjacent

transition rocks of Wales, when the associated volcanic action was in full activity.

2. To the subsequent degradation of the old red sandstone, both when submarine and during its elevation.

3. To various alluvial causes, of date posterior to the desiccation of the old red sandstone, including the erosion of rivers, the deposits of partial lakes, and the accumulation of travertin.

April 9.—A paper was first read, entitled "A short notice of the Coast Section from Whitstable in Kent to the North Foreland in the same County," by William Richardson, Esq., F.G.S.

The author commences his memoir by describing the changes which the line of coast has undergone; and he states that many parts of it, as Hearne Bay and the Old Haven, near Bishopstone, have completely lost the features to which they owe their appellation. He then enters into a minute detail of the London and plastic clays, of which the district is principally composed. He shows that the former constitutes the whole of the cliffs from Whitstable marshes to Hearne Bay, and the upper part of that to the eastward of it. The clay he divides into two portions; the higher contains much sand and green earth, and is occasionally marked with patches and streaks of light blue or green clay; and the lower preserves the usual characters of the formation. The only organic remains which he noticed were fragments of wood, teeth of fishes, and portions of an Encrinite and of *Pentacrinites sub-basaltiformis.* Iron pyrites and selenite are stated to abound in every part of the clay, and amber and jet to be occasionally found in it. A minute description is given of the Septaria, which are said to be very numerous and to have the surface often covered with small ramifications resembling branches flattened by pressure.

Of the plastic clay formation only the sandy portion is said to occur on the coast. It first appears to the east of Hearne Bay rising from beneath the London clay at an angle of about 5°, and extends to Birchington, where it is succeeded by chalk. It contains beds of pebbles and friable sandstone inclosing shells. Sectional lists are given of the cliff near Bishopstone, and of that on which the Reculver Towers are situated; and the author rectifies the error in the "Outlines of England and Wales," where it is stated that the Reculver cliff is composed of London clay. The fossils mentioned in the paper are confined to the genera Venus, Cerithium and Trochus, and the remains of fishes. The chalk which forms the coast from Birchington to the North Foreland is mentioned only for the purpose of showing that it declines to the westward at the same angle as the superior formations. A bed of brown loam containing a few flints is described as covering the surface of the London and plastic clays, and to be thickest in those parts where the cliffs rise to the greatest height. The vast accumulation of bones in the oyster-bed opposite Swale Cliff is also described, and among those obtained by the author are the remains of the elephant, horse, bear, ox and deer.

A paper was afterwards read, "On the several Ravines, Passes, and Fractures in the Mendip Hills and other adjacent Boundaries of the Bristol Coal-field, and on the geological Period when they were effected," by the Rev. David Williams, F.G.S.

The district included in this memoir comprehends the western portion of the Mendip Hills, and the western and north-western boundaries of the Bristol coal-field. It presents a series of lofty ridges composed of mountain limestone and traversed by deep transverse ravines which connect the valleys on the opposite sides of the ridges. The author describes, in great detail, the characters, both physical and geological, of each ravine; and shows that in consequence of their being filled, in part, with horizontal beds of dolomitic conglomerate, red marl, and lias, they were formed previously to the deposition of those formations; but he also shows that the ravines have been subsequently acted upon by a body of water, forming what he terms valleys of denudation in valleys of elevation.

In describing the defile of St. Vincent's Rocks, near Bristol, the author states, that he has discovered two lines of fracture independent of the one noticed in the memoir of Dr. Buckland and Mr. Conybeare on the south-western coal-field*. He says that the principal evidence which he has of the existence of these two faults, rests on the beds of clay, belonging to the lower limestone shale, occurring twice on both sides of the ravine between the first fault and the commencement of the shale beds in their true position beneath the mountain limestone.

In alluding to the bone-caves and fissures of the Mendip Hills, the author conceives that they were formed contemporaneously with the ravines; and that he had found among their organic contents the remains of the Mastodon.

April 23.—A paper was first read, "On the Tertiary Formations of the Kingdom of Murcia, in Spain," by Charles Silvertop, Esq., retired Brigadier in the Spanish Service, K. R. O. C. III., F.G.S.

This memoir is a continuation of papers on the tertiary formations of the South of Spain, read before the Society during the last and preceding Sessions†. The district described is situated in the south-eastern portion of the kingdom of Murcia, and consists of extensive plains and valleys of tertiary formations, bounded by discontinuous ridges of mica slate, transition rocks, and nummulitic limestone. The tertiary deposits the author divides into four districts, which he names from the principal towns situated in their immediate neighbourhood, viz. Lorca, Totana, Alhama and Mula, and Cartagena.

The tertiary strata of Lorca he separates into two systems, one characterized by the beds being horizontal, the other by their being highly inclined. The horizontal beds consist of reddish friable

* Geol. Trans., 2nd Ser., vol. i. p. 241.

† See Phil. Mag. and Annals, N.S., vol. vii. p. 453; vol. viii. p. 150; Lond. and Edinb. Phil. Mag. vol. iii. p. 370.

sandstone and greyish marl. In the sandstone the author noticed no organic remains; but near the eastern boundary of the district he observed, in a mass of clay mixed with sand, innumerable small oysters; and in a yellowish, calcareous freestone, corals, clypeasters, pectens, and oysters. The best points for examining the argillaceous beds are stated to be in the ravines between Lorca and Velez Rubio, where they consist of about fifty feet of marl, containing a few thin strata of reddish pulverulent sandstone, and inclose shells belonging to the genera Pecten, Ostrea, Venus, Tellina, Murex, Emarginula, &c. Upon the argillaceous strata rests a bed of conglomerate in which the author found the long-hinged oyster, so abundant in the newer formations of the South of Spain. The inclined system occurs in the immediate vicinity of Lorca, and consists in the lowest part of sandy loam and sandstone, calcareous and quartzose freestone, and fine conglomerate; and in the upper part of foliaceous, indurated marl, gypseous marl, and gypsum. No organic remains were noticed by the author. The strata dip towards the north at an angle of 15 or 20 degrees, and rest upon the highly-inclined beds of transition rocks.

Totana. This village is about twenty miles E.N.E. of Lorca, and is situated in a prolongation of the highly-inclined gypseous system of the latter place.

Alhama and Mula. In the immediate neighbourhood of Alhama no tertiary strata occur, but to the north of the plain extending from it to Murcia, is a hilly tract in which is situated Mula. The southern part of this tract is composed of an immense deposit of earthy, whitish-grey, argillaceous marl, containing numerous beds of gypsum and brine springs, and the northern of a thick series of sandstone and argillaceous beds which are slightly inclined towards the south and dip under the former. In the alternating sandstone and argillaceous beds, portions of the long-hinged oyster were found. Beneath this series, in the neighbourhood of Mula, occur beds of reddish sandy loam and sandstone, resting on highly inclined strata of nummulitic limestone; and to the north of this ridge of limestone is an horizontal deposit of the tertiary, shelly freestone before described. Along the south-eastern border of the hilly district, the gypseous formation is disturbed in the same manner as at Lorca and Totana.

Cartagena. In the neighbourhood of this town the tertiary strata are extensively developed, and constitute, apparently, the whole of the great plain which ranges from Cartagena northwards to the Fuensanta ridge. On the southern extremity of the plain the strata dip towards the north, and at the northern extremity towards the south. The surface of the district consists of clay, marl and sand, which the author conceives have been derived from the decomposition of the tertiary strata. The beds from the eastern termination of the Fuensanta ridge to the Segura, consist of the calcareous sandstones and comminuted shelly limestone; and in the neighbourhood of Cartagena the same beds are well displayed in numerous and extensive quarries.

The author in conclusion states that M. Deshayes considers the tertiary deposits of this portion of Murcia to belong to the second and third epochs.

A memoir was afterwards read, "On the Geology of the Bermudas," by Lieut. Nelson, of the Royal Engineers; and communicated by the President.

The author commences the memoir with a general description of the form, structure, and meteorological phenomena of the Bermudas, and draws a minute comparison between the characters which they present, and those assigned by Kotzebue to the Coral Islands in the Pacific.

He says that the Bermudas consist of about one hundred and fifty islets, lying within a space of fifteen miles by five, and situated on the S.E. side of a zone of coral reefs, approximating in form to an ellipsis, the *major* axis of which is twenty-five miles and the *minor* thirteen. The highest point is stated to be Sears Hill, 260 feet above the level of the sea; and the undulating portions of the islands are described as resembling sand-hills in shape, and chalk downs in colour.

The whole group is composed of calcareous sand and limestone, derived from comminuted shells and corals, and the different varieties are associated without any definite order of position, the harder limestones occasionally resting upon loose sand. The arrangement of the beds is often domed-shaped, but in many instances the strata are singularly waved.

The bottom of the basin within the zone of coral reefs is stated to consist of corals, calcareous sand, and soft calcareous mud resembling chalk, and considered by the author to have been derived from the decomposition of Zoophytes.

Under the head of encroachments, he describes the banks of detritus thrown up by the sea, and the progress which, under certain circumstances, the loose sand makes in overwhelming tracts previously fertile. He states that wherever the shrubs and creepers have been destroyed, the sand has spread rapidly, but that it is invariably stopped as soon as it arrives at a plantation or row of trees.

The shells found by the author in the sand as well as the limestone, belong entirely to recent species, the most abundant being the *Venus Pennsylvanica*, which in some of the islands constitutes entire beds. The only vegetable remains which he observed were casts of the root of a plant considered by the natives to belong to the palmetto which now grows upon the island.

Caverns are stated to be very numerous, and their origin is assigned to the undermining action of the sea.

The memoir concludes with an account of the method by which the author conceives coral islands and reefs are formed.

May 7.—A paper was first read, "On the Distribution of Organic Remains in the Lias Series of Yorkshire, with a View to facilitate its Identification by giving the Situation of its Fossils," by W. Williamson, jun., Esq. of Scarborough.

The part of the Yorkshire coast to which this paper immediately

refers, extends from the Peak Hill near Robin Hood's Bay to the village of Saltburn near Redcar. The lias presents the threefold arrangement of alum-shale, marlstone, and lias marl or lower lias rock.

The first of these deposits, the author says, consists of three distinct divisions, viz. (*a*) soft, rubbly shale 130 feet thick, (*b*) hard shale breaking into large lamellar blocks, 30 feet thick, and (*c*) soft sandy shale from 15 to 20 feet thick. The upper part of the superior bed of soft shale (*a*) is characterized by *Ammonites striatulus, A. communis, A. crassus,* and *Trigonia literata*; the middle portion, from which alum is manufactured, by *Ammonites Walcottii, A. heterophyllus,* and *Nautilus astacoides*; and the lowest by *Ammonites exaratus, A. elegans, Nucula ovum,* and the remains of Saurians. The hard shale (*b*) is distinguished by the presence of jet, *Ammonites elegans, Belemnites compressus, B. tubularis,* and *Inoceramus dubius*; and the lower bed of soft shale (*c*) by the great abundance of *Ammonites annulatus.*

The marlstone, or second division of the lias series, is characterized by *Ammonites Hawkerensis, A. Clevelandicus, A. Stokesii, Belemnites conicus, B. elongatus, Turbo undulatus, Dentalium giganteum, Isocardia lineata, Cardium multicostatum, C. truncatum, Corbula cardioides, Amphidesma recurvum, Mya V-scripta, M. literata, Plagiostoma læviusculum, Pecten equivalvis, P. sublævis, Avicula inæquivalvis, A. cygnipes, Plicatula spinosa, Modiola scalprum, M. Hillana,* and *Terebratula bidens, T. subrotunda, T. tetrahedra,* and *T. triplicata.*

The lower lias rock is distinguished by *Ammonites planicosta, Plicatula spinosa, Hippopodium ponderosum, Lutraria ambigua, Pinna folium, Gryphæa depressa, G. Maccullochii, G. incurva, Pentacrinites Briareus,* and *P. vulgaris.*

In preparing these lists, the author says that he has omitted to mention those fossils which he has not seen, or those which, from their rarity, can be of little use in distinguishing the different subdivisions of the lias series; and in conclusion, he states, that he has found the fossils enumerated above, almost invariably in the beds to which he has assigned them; and he is of opinion that similar sections may be drawn up on the same minute scale of the contents of all the other strata, but that it remains for further investigation to determine to what extent.

A paper was afterwards read, entitled, "Observations on the Loamy Deposit called Loess in the Valley of the Rhine," by C. Lyell, Esq. For. Sec. G.S.

In this paper Mr. Lyell details some observations made by him in the summer of 1833, on the loess between Cologne and Heidelberg, and in several parts of Baden, Darmstadt, Würtemberg and Nassau. Near Bonn large deposits of loess containing recent shells rest on the gravel of the plain of the Rhine. The author collected two hundred and seventeen entire shells, of which one hundred and eighty-five individuals were of terrestrial species belonging to the genera Helix, Pupa, and Clausilia, and thirty-two of

aquatic species of the genera Limnea, Paludina and Planorbis. This large proportion of land shells is very general in the formation. The author then made a collection of such shells as are now drifted down by the Rhine and occasionally cast ashore by the waves, in which case the shells for the most part retain their colour and are perfectly distinguishable from fossils washed out of the loess. Out of two hundred and seventy-three individuals thus procured, one hundred and forty-seven were land shells and one hundred and twenty-six aquatic. The author infers that if the waters of the Rhine were now received into a lake, the sediment of such a lake might contain more terrestrial than aquatic shells.

After some observations on the hollows and furrows in the gravel of the Rhine which have been filled with loess, the author states that the interior of the crater of the volcanic mountain called the Roderberg is in great part filled up with pure loess, which was pierced to the depth of 65 feet in digging a well in 1833. But although this and other sections prove the posteriority of the loess in general to the volcanic formations of the Eifel, Mr. Lyell admits that at Andernach there have been considerable falls of pumice, scoriæ and volcanic sand during the period of the formation of the loess. In proof of this, the sections in the Kirchweg near Andernach, are described.

The loess is then stated to be spread almost everywhere over the tertiary and secondary strata around Mayence, Oppenheim, Alzey, Flonheim, Eppelsheim and Worms. There is a section of loess with shells, alternating several times with gravel, at the Manheim gate of Heidelberg.

The loess between Heidelberg and Heilbronn appears to attain the height of seven or eight hundred feet above the sea. In this district, shells of the *Succinea elongata* alone are so abundant as to exceed in number all the accompanying land shells. The author then mentions the loess near Stuttgard and between Göppingen and Boll in Würtemberg. He found no traces of it in the course of a tour by Heidenheim, Solenhofen, Nuremberg, Bayreuth, and the cave district of Muggendorf, but he found it again between Bamberg and Wurtzberg in the valley of the Mayne. It was wanting in the Spessart and the country around Aschaffenberg, but is abundant near Frankfort and in several parts of Nassau. In the valley of the Lahn near Limburg, it contains its usual shells and alternates frequently with gravel.

From these facts and others mentioned in the paper, the author deduces the following conclusions:—

First, That the loess is of the same mineral nature as the yellow calcareous sediment with which the waters of the Rhine are now commonly charged.

Secondly, The fossil shells contained in the loess are all of recent species, consisting partly of land and partly of freshwater shells.

Thirdly, The number of individuals belonging to land species usually predominates greatly over the aquatic, and this seems now to be the case with the modern shells drifted down by the Rhine.

Fourthly, Although the loess when pure appears unstratified, it must have been formed gradually, as the shells contained in it are numerous and almost all entire, and beds of shelly loess sometimes alternate with strata of gravel or volcanic matter.

Fifthly, Some volcanic eruptions must have taken place during and after the deposition of loess.

In conclusion, the author states that great changes must have occurred in the physical geography of the basin of the Rhine since some of the loess was deposited, and consequently at a comparatively modern geological era, when the recent Testacea existed.

As the waters must have been at rest when the loamy sediment was thrown down, we must suppose one or many temporary lakes and ancient barriers which have since been removed. It is shown that to assign the probable places of these would be very difficult, and more data are required respecting the greatest height which the loess attains.

May 21.—A paper was read, "On certain Trap Rocks in the Counties of Salop, Montgomery, Radnor, Brecon, Caermarthen, Hereford and Worcester; and the Effects produced by them upon the stratified Deposits," by Roderick Impey Murchison, Esq., V.P.G.S. F.R.S. &c.

Having established an order of succession in the various sedimentary formations between the carboniferous series and the older grauwacke slates, the author proceeds in this memoir to explain the nature of the trap rocks which rise to the surface in the region under review. These rocks are described in the following order.

1st. Those which protrude in separate ridges through various members of the grauwacke series between Lilleshall Hill, Salop, on the north-east, and Llangadock, Caermarthenshire, on the south-west.

2nd. The Malvern and Abberly Hills, including dykes which traverse the old red sandstone.

3rd. Rocks penetrating the coal measures.

I. *Shropshire and Montgomeryshire.*—Lilleshall Hill consists chiefly of compact felspar rock, having in parts a sienitic structure. The Wrekin, which has been described by Mr. A. Aikin in the Geological Transactions *, is of nearly similar composition. Lea and other rocks near Wrockardine have the same base, but pass into porphyry and clinkstone.

In Charlton Hill, porphyries and greenstones occur where they had not been previously noticed.

These rocks mark parallel axes of different lengths on the north bank of the Severn, ranging from north-east to south-west, and piercing through beds of grauwacke, chiefly those of the third, or Horderley and May Hill † formation, the sandstones of which, on the sides of the Wrekin and Arcal, and at Charlton Hill, are converted into quartz rocks at their points of contact with the trap.

The line of disturbance occasioned by the protrusion of the zone

* Geol. Trans., 1st Series, vol. i. p. 191.

† See the "Table of the stratified Deposits," &c., in our last volume, p. 370.

of Charlton Hill, is traced in certain trap rocks which appear in the bed of the Severn, near Cound, extending thence to the south-west.

Caer Caradoc.—This remarkable ridge, formerly described by Mr. A. Aikin*, is on a line of eruption parallel to that of the Wrekin. It includes the hills of Lawley, Great and Little Caradoc, Helmeth, Ragleath, &c., together with a large *contrefort* on its south-eastern face. At Cardington and Hope Bowdler it consists of many varieties of felspar rock, sienite and greenstone. The beautiful amygdaloid with actynolite first pointed out by Dr. Townson, and supposed to be peculiar to one spot, is shown to be of frequent occurrence in several of these hills. The axis of elevation of this ridge has been traced by the author to nearly six miles south-west of the limit formerly assigned to it, the trap reappearing at Wartle Knoll, Hopesay, Sibdon, Aston and Corston.

Numerous examples are adduced of the conversion of sandstone into quartz rock where in contact with the eruptive rocks of these hills, particularly on the south-eastern flank of the Lawley, Little Caradoc, and Cardington Hills, the strata of sandstone being thrown off the flanks of the trap rock in vertical and dislocated forms, in some of which the traces of bedding are with difficulty observed. These conversions of sandstone on the sides of the Wrekin and Caradoc Hills are supposed to have been caused by the action of heat, accompanying the forcible intrusion of some of these trap rocks. A large mass of impure limestone, somewhat indurated, and containing many fossils of the Ludlow formation, has been heaved into a vertical and detached position at Botville, on the north-western face of Caer Caradoc.

Besides these dislocated masses, the author describes a portion of the third formation (May Hill and Horderley), which rises from beneath the Wenlock shale, contains organic remains, and reposes on the flanks of these trappean hills, as being in structure so analogous to the unstratified rocks themselves, that he terms it "*Volcanic Sandstone*," and conceives that it must have been formed during a period of volcanic action, and that the materials of which it is composed are the residue of ashes given off during submarine ejections. A similar rock is found near the Wrekin.

Longmynd, Linley, Pontesford, and Haughmond Hills.—The ancient grauwacke system of the Longmynd and Linley Hills, or mineral axis of Shropshire†, is penetrated by a vast number of points of eruptive trap, chiefly greenstones. Pontesford Hill, amid varieties of greenstone, contains also a porphyry and a remarkable amygdaloid. This zone is reproduced at intervals in Sharpstone Hill, and has been observed by the author in Haughmond Hill, four miles north-east of Shrewsbury, where it passes into a coarse sienite. Vertical, veined, and indurated strata appear at different points along these lines of eruption. Copper ores have been partially worked on the western sides of the Longmynd, and at Norbury. Quartz crystals and small portions of anthracite are found

* Geol. Trans., 1st Series, vol. i. p. 207.

† See our last volume, p. 370.

here and there, and at Lyds Hole are jaspidified red sandstones, with abundant veins of carbonate of lime, &c., the whole being in a state of extreme contortion and irregularity.

Shelve, Corndon, &c.—The mining district of Shelve is an isolated tract, separated from the Linley and Longmynd Hills by the remarkable ridge of quartz rock, called the Stiper stones. It is made up of parallel ridges of trap, and alternating depressions in grauwacke. The trap rocks of this district are greenstones, porphyries, claystones, &c. These are separated by the author into two classes; the one, alternating conformably with the strata of the third and fourth grauwacke formations, is supposed to be contemporaneous with them; the other is shown to be posterior to the consolidation of the stratified deposits. In the contemporaneously formed traps are several varieties, some of which, although aggregates of compact felspar with a concretionary structure, contain organic remains: others graduate into the class of volcanic sandstone. The first are best seen near Leigh Hall, the latter in the Corndon flagstones, Mary Knoll dingle, &c., where they are largely quarried.

The other class, or the intrusive trap, rises to the greatest heights and to the largest masses in the Corndon, Stapely, Taudley, and Roundton Hills, which form the chief axis of the district; but there are other linear eruptions, many of which are of extreme tenuity, occasioning numerous alternations of trap and grauwacke. The trap rocks consist of greenstones, porphyritic greenstone, compact felspar, concretionary felspar simple and porphyritic, amygdaloids, &c.

Some remarkable parallel dykes are described running from north-east to south-west between beds of grauwacke, shale, and calcareous flag, which latter in some instances is converted near the prismatic ends of these dykes, into cream-coloured porcellanite. In the grauwacke adjacent to the trap are also many productive veins of lead, which are respectively described at the Snailbach, Penally, Bog, Gravel, Grit, and Roman Mines.

Besides these ores of lead, sulphate of barytes, sulphuret of iron, and carbonate of lime are very abundant.

Stiper Stones.—This dentated and lofty ledge of quartz rock belongs to sandstones of the fifth formation of the previous table*, which have been altered by igneous action, and thrown up into their present highly inclined and broken forms by the eruption of volcanic rock, lines of which are pointed out both on the western and eastern faces of the ridge, and some of which it is presumed lie concealed beneath it.

To the west and south of the above district small portions of trap are noticed at Nantcribba, Montgomery, and at Heblands near Bishop's Castle.

Breiddin Hills.—These hills are divisible into ridges running from east-north-east to west-south-west, which, although parallel to

* See the Table.

each other, are not parallel to those previously mentioned. The eastern or chief of these is marked at one extremity by the Middleton and Moel y Golfa Hills, and at the other by those of Builthey and Bauseley. In this ridge are compact felspar rocks, slaty porphyries, greenstone, and much concretionary rock, frequently of very large size. These trap rocks burst through strata of grauwacke, in some of which fossils of the Ludlow and Wenlock formations have been found. The strata in contact are much hardened and fractured, and contain many veins of carbonate of lime, and sulphate and carbonate of barytes, &c. &c.

The other chief ridge, or that on which Rodney's Pillar stands, is composed in great part of coarse-grained columnar greenstone, passing at its extremities into fine concretionary trap and clinkstone. The Criggan, a third or minor ridge, included between these, exhibits bosses of greenstone, with altered and silicified schists on their sides.

A new locality of singularly spotted trap rock piercing through an impure limestone full of shells, is noticed at the Cefn, between the Moel y Golfa and Welch Pool.

The celebrated quarries of building-stone at Welch Pool expose a broad dyke of columnar greenstone, passing in one part into concretionary trap traversing strata of the same age, and producing great changes in them in contact. The direction of this great dyke is in the prolongation of the volcanic axis of the Breiddin Hills. The last traces of these concretionary traps are observable in Powis Castle Park.

Radnorshire.—The trap rocks in Radnorshire run in distinct ridges from north-east to south-west: the most eastern are those near Old Radnor; the central or chief masses range from Llandegley and Llandrindod to Builth; a third and unimportant ridge occurs at Baxter's Bank, five miles north-west of Llandrindod.

Old Radnor Group.—These trap rocks occupy two parallel ridges comprising Stanner Rocks, Worsel Wood, and Hanter Hill on the south-east, and Old Radnor Hill and its dependencies on the north-west. These rocks are distinguishable from all others mentioned in this memoir by the abundance of hypersthene, and exhibit many passages from a coarse crystalline hypersthene rock to a fine-grained greenstone. Unlike the hypersthene rocks in the Isle of Skye, their base is for the most part of compact felspar, which passes into granular felspar, porphyry, &c.

In Old Radnor Hill, besides hypersthene rock and greenstone, there are concretionary traps, bastard serpentine, &c. Towards Harpton Court these latter rocks throw off a peculiar conglomerate, having a base of felspar inclosing pebbles of quartz, which the author supposes to have been formed by a mixture of volcanic matter with submarine detritus.

The eruptive masses of trap tilt the strata of the Ludlow formation on the one side, and those of the Wenlock rocks on the other, the most interesting phænomena being observable near Old Radnor church and the quarries to the south of it, where the limestone has

been cleared away from the abrupt faces and points of the intrusive rock. Bands of imperfect serpentine are frequent between the trap and the limestone. The latter, near the contact, is wholly unstratified, crystalline, and hard, and only resumes its ordinary appearance at a certain distance from the trap. The shale is altered into a hard slaty substance. Coatings and nests of anthracite, together with minute veins of copper ore and iron pyrites, appear near the junction, sometimes running from the trap into the altered deposits.

The author states that the phænomena are very analogous to those of the Val di Fassa in the Tyrol.

On the north-eastern prolongation of this line of eruption is the limestone of Nash Scar, which, although at its extremities (Woodside and Corten,) is demonstrably the equivalent of the Wenlock and Dudley rocks, yet in this central part is a craggy mass of altered and crystalline limestone, the changes in which are doubtless due to the igneous influence which has shown itself on the same line at Old Radnor.

Llandegley, Llandrindod, and Builth Group.—This large trappean district having a length of nearly ten miles and a breadth of five, is very similar in structure and physical features to that of Shelve and Corndon in Shropshire, consisting of sharp ridges of trap and deep trenched valleys in grauwacke, all running from north-east to south-west. It is further analogous in presenting some evidence of volcanic eruptions of date contemporaneous with the sedimentary deposits containing the *Asaphus Buchii.* Transverse sections near Gelli and Buries, illustrate these phænomena of repeated conformable alternation of concretionary felspar and other rocks of igneous origin with stratified shelly deposits.

The most prominent elevations of trap consist of greenstones of many varieties, felspar rock with quartz crystals, porphyries, both amorphous and slaty, passing into porphyritic greenstone, amygdaloids of various characters, concretionary rocks, claystone, &c.

The author points out, in some detail, the reckless folly which has led people in this district to seek for coal by driving horizontal galleries through the black schist on the sides of these eruptive ridges, endeavours which they have been led to persevere in from the occasional presence of small pieces of anthracite near the junctions.

The altered strata of grauwacke within and around the exterior of these hills are of too frequent occurrence even to be named in an abstract. Among a great number of cases, the author calls particular attention to the south-western termination of the great mountain of Carneddau near Builth, where certain lower ridges of greenstone, &c., cut through the shale and calcareous flag containing the *Asaphus Buchii,* the beds of which are distorted, broken, indurated and silicified, and in some instances changed to a milk white colour, and to a brittle condition resembling some sorts of porcelain. Numerous and large crystals of iron pyrites occur in these altered beds, and as the mineral waters of the Park wells issue from them, their origin is supposed to be due to the decomposition of the crystallized pyrites. Hence the author infers that the mineral sources

of Builth, Blaen Eddw, Llandegley and Llandrindod, which severally issue from pyritized beds of grauwacke at points of contact with intrusive rocks, are as much the result of volcanic action as the mineral veins of Shelve in Shropshire.

Baxter's Bank contains a coarse-grained greenstone throwing off black shale, which, in contact, is a silicified schist. Galleries in search of coal have also been driven through these inclined strata. A little spur of trap reappears at Caerfagie, one mile south-west of Baxter's Bank.

[To be continued.]

ZOOLOGICAL SOCIETY.

May 13.—A Note was read from Mrs. Barnes, in which it was stated that that lady had brought up from the nest two of the smallest species of Jamaica *Humming-birds.* They were so tame, that at a call they would fly to her, and perch upon her finger. Their food was sugar and water. During the passage to England one of them was killed by the cage in which they were kept being thrown down in a storm; its companion drooped immediately, and died shortly afterwards.

It was remarked that injury to the bird in consequence of such an accident might be prevented by the introduction of a gauze-net screen into the cage, at some little distance within the wires.

Specimens were exhibited of several *Mammalia* from India, which had recently been presented to the Society by Lord Fitzroy Somerset. They were brought under the notice of the Meeting by Mr. Bennett, who called particular attention to the skin of a *Paradoxurus,* which he regarded as that of *Par. prehensilis,* Gray, a species hitherto known only by a drawing of Dr. Hamilton's preserved in the East India House*.

The general colour of the animal is a pale greyish brown, in which longer black hairs are sparingly intermixed on the sides. On the back of the head and neck, and along the middle line of the back, these black hairs are almost the only ones that are visible. On the loins they form three indistinct black bands, of which the lateral are in some measure interrupted. The head is brownish, with the usual grey mark both above and below the eyes, and there are some short grey hairs between the eyes and across the forehead. The limbs are brownish black, rather darker towards their upper part. The tail, at its base, is of the same colour as the back, and rapidly becomes black; its terminal fifth is yellowish white. The ears are rather large, and sparingly covered with short brownish hairs.

Specimens were exhibited of three species of *horned Pheasants,* including the *Tragopan Temminckii,* Gray. In illustration of the history of the latter bird, Mr. G. Bennett, Corr. Memb. Z.S., placed upon the table drawings of specimens observed by him at Macao, and showing the remarkable wattle in various degrees of development. He also read a note on the subject.

In its contracted state the membrane has merely the appearance

* See Lond. and Edinb. Phil. Mag., vol. i. p. 399.

of a purple skin under the lower mandible; and it is even sometimes so much diminished in size as to be quite invisible. It becomes developed during the early spring months or pairing season of the year, from January to March, when it is capable of being displayed or contracted at the will of the bird. During excitement it is enlarged, falls over the breast, and exhibits the most brilliant colours, principally of a vivid purple, with bright red and green spots: the colours vary in intensity according to the degree of excitement. When they are most brilliant, or, in other words, when the excitement is great, the purple horns are usually elevated. The living specimens seen by Mr. G. Bennett were procured from the province of Yunnan, bordering on Thibet. Mr. Beale, in whose aviary at Macao they were, had not succeeded in obtaining females of this race. Its Chinese name is *Tu Xou Nieu.*

Mr. G. Bennett also read a note on the habits of the *King Penguin, Aptenodytes Patachonica,* Gmel., as observed by him on various occasions when in high southern latitudes. He described particularly a colony of these birds, which covers an extent of thirty or forty acres, at the north end of Macquarrie Island, in the South Pacific Ocean. The number of *Penguins* collected together in this spot is immense, but it would be almost impossible to guess at it with any near approach to truth, as, during the whole of the day and night, 30,000 or 40,000 of them are continually landing, and an equal number going to sea. They are arranged, when on shore, in as compact a manner and in as regular ranks as a regiment of soldiers; and are classed with the greatest order, the young birds being in one situation, the moulting birds in another, the sitting hens in a third, the clean birds in a fourth, &c.; and so strictly do birds in similar condition congregate, that should a bird that is moulting intrude itself among those which are clean, it is immediately ejected from among them.

The females hatch the eggs by keeping them close between their thighs; and, if approached during the time of incubation, move away, carrying the eggs with them. At this time the male bird goes to sea and collects food for the female, which becomes very fat. After the young is hatched, both parents go to sea, and bring home food for it; it soon becomes so fat as scarcely to be able to walk, the old birds getting very thin. They sit quite upright in their roosting-places, and walk in the erect position until they arrive at the beach, when they throw themselves on their breasts, in order to encounter the very heavy sea met with at their landing-place.

Although the appearance of *Penguins* generally indicates the neighbourhood of land, Mr. G. Bennett cited several instances of their occurrence at a considerable distance from any known land.

The Secretary announced the recent addition to the Menagerie of the *Perdix sphenura,* Gray; the *Philippine Quail, Coturnix Sinensis,* Cuv.; and the *Hemipodius Dussumieri,* Temm.?: all presented to the Society by John Russel Reeves, Esq., of Canton. He added, that a second male specimen of the *Reeves's Pheasant, Phasianus veneratus,* Temm., had also been sent to the Menagerie by John Reeves, Esq. A pair of the middle tail-feathers of the last-named bird, measuring

upwards of five feet in length, and presented by Wm. Craggs, Esq., were exhibited.

Numerous specimens were exhibited from Mr. Cuming's collection, in illustration of a Paper by Mr. Broderip, entitled, "Descriptions of several New Species of *Calyptræidæ.*"

The following are the new species distributed and characterized in this paper:

Subgenus Calyptræa: Cal. *rudis, corrugata, varia* (a very variable species, allied to *Cal. equestris,* and taking almost every shape which a *Calyptræa* can assume. It differs in thickness according to localities and circumstances), *cepacea,* and *cornea.*

Subgenus Calypeopsis, *Less.* Cal. *radiata, imbricata, lignaria* (The majority of individuals of this species have their shells so deformed that they set description at defiance: the comparatively well-formed shell occurs so rarely that it may be almost considered as the exception to the rule. When in this last-mentioned state, the circumference of the shell is an irregular, somewhat rounded oval, and it rises into a shape somewhat resembling the back of *Ancylus,* with the *apex* very sharp and inclining downwards. The shell in this shape is generally less corrugated than it is in deformed individuals, though some of those are comparatively smooth; but in both states the shell is striated immediately under the *apex,* and is for the most part corrugated on the other side of it.), *tenuis, hispida, maculata,* and *serrata.*

Syphopatella, *Less.*? Cal. *sordida, Unguis, Lichen, mamillaris, striata,* and *conica.*

Subgenus Crepipatella, *Less.* Cal. *foliacea* (bears no remote resemblance to the upper valve of some of the *Chamæ* when viewed from above), *dorsata* (the back of this shell is not unlike the upper valve of some of the *Terebratulæ*), *dilatata, strigata* (may possibly be a variety of the last), *Echinus* (old specimens bear a great resemblance to the figure of *Crepidula fornicata* in Sowerby's Genera of Shells, No. 23, f. 1.), *Hystrix* (approaching the last: but Mr. Broderip would not be positive that they are not all varieties of *Crepidula aculeata,* Lam.), and *pallida.*

Subgenus Crepidula, *Less.* Crep. *unguiformis.* Cal. *Lessonii* (will remind the observer of the upper valves of some of the *Chamæ*), *incurva, excavata* (remarkable for the depth of the internal margin before it reaches the *septum;* that depth, however, being less than in *Crep. adunca,* Sow.), *arenata* (approaches *Crep. porcellana,*) *marginalis,* and *squama.*

May 27.—A Letter was read, addressed to the Secretary by Sir R. Ker Porter, Corr. Memb. Z. S., dated City of Caracas, April 7, 1834. It related chiefly to a *Monkey,* and to some *Tortoises,* recently presented to the Society by the writer.

The *Monkey* is described in detail. It is the *Pithecia sagulata,* the *jacketed Monkey* or *Simia sagulata* of Dr. Traill. Sir R. Ker Porter points out the several differences in colouring which exist between this individual and the published description by the Baron Humboldt of the *Pithecia Chiropotes*: these consist chiefly in the comparative paleness of its back, and the greater darkness of the remainder of its

body and of its bushy beard. He adds that the animal drinks frequently, always bending down on its hands, and putting its mouth to the surface of the water, heedless apparently of wetting its beard, and indifferent to the observations of lookers-on: he never saw it take up water in the hollow of its hand, and carry it in this manner to its mouth in order to drink. Its favourite fruit is the apple; and it does not refuse the pinion of a roasted chicken. Its voice is a weak and chirping whistle, which becomes shrill and loud when the animal is angry. It was obtained from the vicinity of the Orinoco, not far distant from the Rio Negro, in the heart of Guiana. It is known as the *Mono Capuchino.*

The *Tortoises* are referable to the *Testudo carbonaria,* Spix.

The Secretary announced that there had recently been added to the Menagerie a *white-crested Cockatoo, Plyctolophus cristatus,* Vieill.; and a pair of the *blue Jay, Garrulus cristatus,* Cuv.

He also stated that there had been acquired for the Menagerie a *Rhinoceros* of the *one-horned* species of Continental India. It is said to be about four years old. Its height at the loins, the highest part of the back, is 4 feet 10½ inches; its length, from the root of the tail to the tip of the nose, measured in a straight line, is 10 feet 6 inches; its weight is about 26 cwt.

A specimen was exhibited of the young of the *Sandwich Island Goose, Bernicla Sandvicensis,* Vig., which was hatched at Knowsley. It was accompanied by the following note from the President, Lord Stanley.

"Through the kindness of John Reeves, Esq., I received at Knowsley a pair of these birds on the 15th of February, 1834. They did not at first, when turned out on the pond among the other water-fowl, appear to take much notice of each other; but some workmen being at the time employed about the pond, one of the birds (I think, from recollection, it was the male,) seemed to have formed some sort of attachment to one of the men working. Whenever he was present the goose was always near to him, and whenever absent at his dinner, or when otherwise employed, the bird appeared restless, and gave vent to its solicitude by frequent cries, which as well as the anxiety, always ceased with the reappearance of the workman.

"The man having frequently occasion to pass through a door, which was obliged to be kept open, it was feared that the attachment of the animal might lead to its following its friend, and that on its exit, it might fall in with and be worried or stolen by vermin, and in consequence the pair of geese were confined in one of the divisions adjacent to, but divided from, the pond, on February 26.

"Within this small inclosure, in the sheltered half of it, in one corner, stood a small hutch, in which the female on the 5th of March laid her first egg. Till within a few days of that period no alteration took place in their manners, but it then became obvious that the male was jealous of intruders, and would run at and seize them by the trowsers, giving pretty sharp blows with his wings; but this always ceased if he observed that the female was at some distance, when

he would instantly rejoin her: his return to the female was always accompanied by great hurry and clamour, and much gesticulation up and down of his head, but not of the wings. Three other eggs followed on the 7th, 9th, and 11th of March. The eggs were white, and very large in proportion to the size of the bird, being, I should imagine, (for, having no proper scales at hand, I did not weigh or subtract any of them, hoping that more might be laid,) fully equal to those of the *Swan Goose* or *Anas cygnöides*. The goose also surprised us by the rapidity of her operations, for we were hardly aware of the fourth egg having been laid that morning, when it was evident that she had begun to sit. During the whole period of incubation there could not be a more attentive nurse, and indeed she could not well help it, for the male, if she seemed inclined to stay out longer than he thought right, appeared, by his motions, to be bent on driving her back, nor was he satisfied till he had accomplished his object, when he again resumed his usual position, with his body half in half out of the hutch and his head towards the female; but if any person crossed the yard of the division, he would immediately hurry after the intruder, though, if he found there was no intention of molesting the nursery, he seemed generally satisfied, and did not like to quit the sheltered part of the division. At night he constantly made room for himself by the female, the result of which was unfortunate towards the progeny.

"On the 12th of April the eggs began to chip, and on the 13th two goslings were excluded; but it was found that the mother had pushed from under her the other two eggs, which were consequently taken away and put under a hen, though, as one was very nearly cold, little hopes of any success with that were entertained, and it was in fact never hatched, but probably died in consequence of the removal by the goose at an important moment. On the morning of the 14th it was ascertained that she or the male, who always now sat close beside her in the box, had killed one of the two she had at first hatched, for it was found dead and perfectly flat. The fourth egg, which was put under the hen, was assisted out of the shell, and appeared weakly from the first, and as its mother had lost one, we put it to her, in hopes it would do better than with its nurse. She took to it at first very well; but subsequently, both the parents beating it, it was returned to, and well cared for, apparently, by its nurse, but died on the 20th, having received some injury in one eye, either from the old ones, or perhaps from the hen scratching, and thereby hitting it. The remaining gosling is doing very well, and appears strong and lively, and the parents are extremely attentive to it; and I have little doubt but these birds may easily be established, (with a little care and attention,) and form an interesting addition to the stock of British domesticated fowls.

"In its general appearance, and its Quaker-like simplicity of plumage, it seems to approximate most to the family of the *Bernacles*; but it appears to have almost as little (if as much) partiality for the water as the *Cereopsis*."

The bird in question was named by Mr. Vigors at the Meeting of

the Society on June 11, 1833. (Lond. and Edinb. Phil. Mag., vol. iii. p. 293.) It may be characterized as follows:

BERNICLA SANDVICENSIS. *Bern. brunneo-nigrescens, subtùs marginibusque plumarum pallidioribus; collo albescenti; gulâ, facie, capite supernè, lineâque longitudinali nuchali nigris; crisso albo.*

Long. tot. 24 unc.; *rostri*, rictus, $1\frac{1}{4}$; *alæ*, $13\frac{1}{2}$; *caudæ*, 5; *tarsi*, $2\frac{1}{4}$.

Hab. in insulis Sandvicensibus, et in Owhyhee.

Mr. Owen read a Paper "On the young of the *Ornithorhynchus paradoxus*, Blum." It was illustrated by drawings of the young animal and of various details of its structure, both external and internal, derived chiefly from the examination of the individual recently presented to the Society by Dr. Weatherhead: this individual was exhibited, as was also a smaller specimen, forming part of Dr. Weatherhead's collection.

The circumstances which first attract attention in these singular objects are the total absence of hair; the soft and flexible condition of the mandibles; and the shortness of these parts in proportion to their breadth as compared with the adult. The tongue, which in the adult is lodged far back in the mouth, advances in the young animal close to the end of the lower mandible, and its breadth is only one line less in an individual four inches in length than it is in fully grown animals; a disproportionate development which is plainly indicative of the importance of the organ to the young *Ornithorhynchus* both in receiving and swallowing its food.

On the middle line of the upper mandible, and a little anterior to the nostrils, there is a minute fleshy eminence lodged in a slight depression. In the smaller specimen this is surrounded by a discontinuous margin of the *epidermis*, with which substance, therefore,—and, probably, from its having been shed, of a thickened or horny consistence,—the caruncle had been covered. It is a structure of which the upper mandible of the adult presents no trace, and Mr. Owen regards it as analogous to the fœtal peculiarity of the horny knob on the upper mandible of the *Bird*. He does not, however, conceive that this remarkable example of the affinity of *Ornithorhynchus* to the feathered class is necessarily indicative of its having been applied, under the same circumstances, to overcome a resistance of precisely the same character as that for which it is designed in the young bird, since all the known history of the *ovum* of *Ornithorhynchus* points strongly to its ovoviviparous development.

The situation of the eyes is indicated by the convergence of a few wrinkles to one point; but the integument is continuous, and completely shrouds the eyeball. In the absence of vision in the young animal, strong evidence is afforded of its being confined to the nest, there to receive its nourishment from its dam; and this deduction is corroborated by the cartilaginous condition of the bones of the extremities, and by the general form of the body: the head and tail are closely approximated on the ventral aspect, requiring force to pull the body into a straight line; and the relative quantity of integument on the back and belly shows that the position necessary for progressive motion is unnatural at this stage of growth.

Mr. Owen describes other external appearances of the young *Orni-*

thorhynchus, and then enters at considerable length into its anatomy. The stomach is nearly as large in an individual four inches in length as in the adult animal. In this specimen it was found filled with coagulated milk, and no trace was visible, on the most careful examination, of worms or bread, on which, up to the time of his discovery of the mammary secretion, Lieut. the Hon. Lauderdale Maule had believed that this individual had been sustained. A portion of this coagulated substance was diluted with water, and examined under a high magnifying power in comparison with a portion of cow's milk coagulated by spirit, and similarly diluted. The ultimate globules of the *Ornithorhynchus's* milk were most distinctly perceptible, detaching themselves from the small coherent masses to form new groups: the corresponding globules of the cow's milk were of larger size. Minute transparent globules of oil were intermixed with the milk globules of the *Ornithorhynchus*. A drop of water being added to a little mucus, it instantly became opake; and its minutest divisions, under the microscope, were into transparent angular flakes, entirely different from the regularly formed granules of the milk of the *Ornithorhynchus*.

In passing in review the several *viscera* of the young *Ornithorhynchus*, Mr. Owen observed on various physiological deductions which might be drawn from them, and on the differences and resemblances borne by them to the same organs in the ordinary viviparous *Mammalia* and in the *Marsupiata**.

ENTOMOLOGICAL SOCIETY†.

June 2.—Numerous donations of books and insects were announced. A prospectus of Prize Essays, on the subject of noxious insects, and remedies for their destruction, proposed by the Society, was read, offering the sum of five guineas annually for the best essay. The subject of that for the present year to be the Turnep-fly.

Papers were read upon the *Sphinx ephemæriformis*, by J. F. Stephens, Esq.; Descriptions of various larvæ of *Coleoptera*, by Mr. G. R. Waterhouse; Upon the habits of *Odynerus Antilope*, by Mr. Westwood; Descriptions of the Irish Species of *Thysanura*, by Mr. Templeton. Mr. Spence alluded to the annoyance caused to the inhabitants of Brighton and some parts of London by the swarms of a minute ant, which had infested houses, occasionally to such a degree that the inhabitants were compelled to quit them.

July 7.—A report was read of the purchases of insects made at the sale of the late Mr. Haworth's collections.

Papers were read upon the British species of *Dromius*, by C. C. Babington, Esq., M.A.; Upon a new British genus of *Neuroptera* and family *Hemerobiidæ*, by J. O. Westwood, F.L.S.; Description of a new genus of *Curculionidæ*, from St. Helena, by M. Chevrolat of Paris; Upon the British genera *Acentria*, *Acentropus*, and *Zancle*, by Mr. Westwood; and the conclusion of Mr. Templeton's *Thysanura Hibernica*.

* Other notices relative to the *Monotremata* and their affinities will be found referred to in our last Number, p. 147.

† Continued from vol. iv. p. 385.

A Committee was appointed to investigate the ravages of the Cane-fly of Grenada, with a view to the suggestion of the most efficacious remedies against its attacks.

Aug. 4.—Lieut.-Col. W. H. Sykes, F.R.S., in the chair. The report of the Committee appointed at the last meeting was read, containing a variety of suggestions which were stated to have been communicated to the Agricultural Society of Grenada. A curious wasp's-nest built in the folds of a piece of paper, was exhibited by Mr. Ingpen. Memoirs were read upon some new species of Ants from the East Indies, with observations upon their habits, by Lieut.-Col. Sykes; Description of a beautiful nondescript *Lamia* from Sierra Leone, by Mr. Westwood.

XXXIV. *Intelligence and Miscellaneous Articles.*

M. BREITHAUPT'S MINERALOGY.

SINCE the insertion of the notice from Professor Hausman in the 26th Number of this Journal, our attention has been drawn to the recent work of another mineralogical writer, Mr. Breithaupt, by two specimens, named by him *peganit* and *pegmatit.* We do not perceive upon what principle he has considered these to be new species, as they appear to us only varieties of the wavellite of Frankenberg. His kupferindig, we recollect, did not present any very definite specific characters to us; and if his other supposed new species are not better established than these, we fear that his many new names will rather embarrass than assist mineralogy, and that his discoveries will belong to a class which, we believe, is denoted in his own language by a term we cannot well translate, but which refers their value to the number of *sacks of wind* they may be worth.

OBSERVATIONS ON THE TEMPERATURE OF ARTESIAN WELLS IN DEGREES OF THE CENTIGRADE THERMOMETER, THE DEPTHS BEING EXPRESSED IN METRES.

In the Neighbourhood of Vienna.

Depth.	Temperature.
73·96	13·75
34·14	11·6
34·14	11·6
18·96	11·25
Mean temperature of the surface	10·25.

Hence the temperature increases at the rate of 1° centigrade in 20·32 metres.

Rochelle.

123·16	18·12
Mean temperature of surface	11·87

Or the temperature increases at the rate of 1° centigrade in 19·71 metres.

Epinay.

67	14·
54	13·3
12	11·

These observations give an increase of 1° centigrade in descending through 18·32 metres.

Kupffer's observations in the mine of Bogoslowsk in the Ural, give an increase of 1° centigrade in descending through 19·84 metres.—*Poggendorff's Annalen*, v. 32.

ON THE PHYSICAL AND THERAPEUTIC PROPERTIES OF CHROMATE OF POTASH. BY M. JACOBSON.

Neutral chromate of potash may be exposed to a very high temperature without being decomposed, unless charcoal be added to it, which renders it incandescent. Hemp, cotton, linen, or cloth, impregnated with this salt become very combustible, and burn with strong and lively incandescence, and with considerable disengagement of light and heat. The oxides of chromium and its different salts possess the same property. The author has employed this property of chromate of potash for the preparation of *moxas*. Those which he made use of were made with blotting-paper soaked in a solution made with one part of this salt and 16 parts of water: the author proposes to make matches by immersing cotton in this solution. An important property of this salt is its great solubility in water, and its power of preserving vegetable and animal matter from putrefaction; it also removes the disagreeable smell from putrid substances.

The bichromate is especially suited for preservation and disinfection, the solution containing about 1·250 of its weight of the salt. Animal substances, with the exception of the nervous parts, are not at all altered by this solution. With respect to the therapeutic properties of chromate of potash, M. Jacobson employs it externally as a resolvent, and when concentrated, as a caustic. Internally, taken in doses of 1 or 2 grains, it is emetic.—*Journal de Chimie Médicale*, Février 1834.

DISCOVERY OF CHRENIC AND APOCHRENIC ACIDS IN THE MINERAL WATERS OF PORTA. BY M. BERZELIUS.

The waters of Porta have acquired great celebrity on account of their medicinal properties. The water is abundant, and bubbles of gas, which consist of 6 volumes of azote and 1 volume of carbonic acid, constantly rise from the bottom of the spring: the temperature of the water is invariably about 45° Fahr. The colour of the water is yellowish, and caused by an organized substance which it is difficult to isolate; it is composed of oxygen, hydrogen, azote and carbon. It possesses acid properties, and when concentrated has a sour taste: it is a mixture of two acids, one of which, occurring in the greatest quantity, Berzelius calls *chrenic acid*, and the other *apochrenic acid*, because it is formed from the first by the influence of oxygen gas, &c. These acids are weak; they nevertheless decompose the acetates if the mixture is evaporated. Chrenic acid does not crystallize: the solution concentrated to the consistence of a syrup is almost colourless. When dried *in vacuo* it splits in all directions, and has a false appearance of being crystallized; its taste is then distinctly acid and astringent. The watery solution has only an

astringent taste; it is soluble in absolute alcohol, and but slightly so in alcohol of density 0·85. The chrenates of the alkaline earths are but slightly soluble in water, and they form insoluble subsalts. The greater part of other chrenates are insoluble except the protochrenate of iron, which is very soluble.

Apochrenic acid is but slightly soluble in water, to which it gives a brownish colour. The apochrenates resemble the chrenates strongly, but they are brown or black, insoluble in alcohol, and combine with hydrate of alumina by digestion, forming a colourless solution. By this method they are easily separated from the chrenates.

These two acids are found in several chalybeate waters in Sweden, even when the waters are colourless. They may be separated from the ochre which these waters deposit by boiling it with hydrate of potash. The alkali being afterwards saturated with acetic acid, the apochrenic acid is to be precipitated by acetate of lead as long as a brown precipitate is formed, or a greenish one, which becomes brown. The liquor afterwards neutralized by an alkaline carbonate precipitates chrenate of copper in greenish white flocks, the quantity of which is increased by adding more acetate of copper. The copper is afterwards separated from the acid by means of sulphuretted hydrogen. Even ochry iron ore contains these acids.

The waters of Porta contain these two acids in the states of the chrenates of soda and ammonia. In 100,000 parts of the water there were found,

Chloride of potassium	0·3398
——— sodium	0·7937
Chrenate of soda	0·6413
Chrenate and carbonate of ammonia	0·8608
Bicarbonate of lime	9·0578
——— magnesia	1·9103
——— manganese	0·0307
——— iron	6·6109
Phosphate of alumina	0·0110
Silica	3·8960
Chrenic and apochrenic acids	5·2515
	29·4038

Berzelius considers the azote disengaged from the water, and the ammonia which saturates the chrenic acid, as the product of the spontaneous decomposition of the two organic acids; and he attributes the acids to the putrefaction of vegetable substances on the surface of the earth, in the extensive marshy forests which surround the spring.—*Journal de Chimie Médicale*, Avril 1834.

SCIENTIFIC BOOKS.

In the Press.

A Guide to Geology; explaining the Elementary Facts and Inferences of that Science, with condensed Descriptions of the principal Stratified and Unstratified Rocks, Tables, Plates, &c. By Professor Phillips.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London, and by Mr. Veall, at Boston.

Days of Month. 1834.	Barometer. London. Max.	Barometer. London. Min.	Barometer. Boston $8\frac{1}{2}$ A.M.	Thermometer. London. Max.	Thermometer. London. Min.	Thermometer. Bost. $8\frac{1}{2}$ A.M.	Wind. Lond.	Wind. Bost.	Rain. Lond.	Rain. Bost.
July 1	30·295	30·157	29·74	73	54	63	E.	E.	...	...
2	30·080	30·035	29·58	73	54	61	E.	E.	...	·04
3	30·118	30·056	29·58	77	54	61·5	E.	calm	...	...
4	30·151	30·108	29·66	80	54	64	E.	calm	...	...
5	30·086	29·935	29·58	81	61	65·5	NE.	calm	0·58	...
● 6	29·987	29·975	29·42	80	59	63	S.	calm	·43	·03
7	29·993	29·928	29·32	80	59	66·5	S.	W.	·02	·04
8	30·017	29·868	29·27	77	53	62	NW.	calm	·02	·19
9	30·135	30·102	29·45	74	51	62	NW.	calm	...	·36
10	30·064	30·031	29·44	75	51	67	S.	calm	...	·13
11	30·062	29·986	29·46	82	42	64·5	S.	calm	...	·10
12	29·845	29·789	29·25	83	56	68	SW.	W.	...	...
☽ 13	29·936	29·869	29·20	78	56	67	SW.	W.	...	...
14	30·139	30·005	29·37	78	53	64	W.	calm	...	...
15	30·223	30·218	29·55	81	55	68·5	W.	calm	...	...
16	30·242	30·182	29·50	86	54	71·5	SW.	calm	...	...
17	30·142	30·014	29·43	94	63	71	S.	calm	...	...
18	29·761	29·558	29·27	67	57	66·5	SW.	E.	1·22	...
19	29·646	29·461	28·85	63	54	64	W.	E.	·60	1·41
○ 20	29·709	29·683	29·10	70	56	62	SW.	W.	·26	·32
21	29·810	29·728	29·19	67	57	68	S.	NE.	·15	·05
22	30·010	29·911	29·35	71	51	67	N.	calm	...	...
23	30·037	30·012	29·43	78	58	67	E.	calm	...	...
24	30·047	30·020	29·40	78	56	63	SE.	N.	...	...
25	30·005	29·999	29·37	79	50	68	W.	NW.	...	...
26	29·926	29·647	29·27	78	51	68	SW.	calm	·12	...
27	29·885	29·634	29·16	70	48	63·5	SW.	calm	·14	·08
☾ 28	29·989	29·983	29·40	74	61	67	S.	calm	·17	·37
29	29·975	29·934	29·38	82	63	68	E.	calm	1·31	·72
30	29·884	29·867	29·25	80	55	69	SW.	calm	·66	...
31	29·834	29·795	29·20	74	60	69·5	S.	calm	·66	...
	30·295	29·461	29·36	94	42	65·8			6·34	3·84

Remarks.

London.—July 1—3. Fine. 4. Slight haze: very hot. 5. Very fine: heavy rain at night. 6. Thunder and lightning with heavy rain. 7. Cloudy: very fine: showers. 8—10. Cloudy and fine. 11. Very hot. 12. Clear and hot. 13—16. Very hot. 17. Cloudless and excessively hot: thermometer 94° in shade, and 130° in sun's rays. 18. Hot and dry E. wind: at noon rain from S.W.: thunder at night, with very heavy rain, falling to the depth of more than an inch. 19. Heavy rain. 20. Cloudy: showery: heavy rain at night. 21. Showery. 22. Cloudy. 23. Foggy: fine. 24, 25. Very fine. 26, 27. Cloudy: rain at night. 28. Overcast: thunder and lightning, with rain at night. 29. Sultry, with showers: thunder and heavy rain at night. 30. Heavy rain: sultry: clear and fine. 31. Heavy rain: foggy. More rain fell in this month than in the five preceding taken together.

Boston.—July 1. Cloudy: rain P.M. 2, 3. Cloudy. 4, 5. Fine. 6. Cloudy: rain early A.M. and P.M. 7. Cloudy. 8. Rain: rain early A.M. 9. Cloudy: rain P.M. 10. Fine: rain A.M. 11—13. Fine. 14. Cloudy. 15. Fine. 16. Cloudy. 17. Fine. 18. Stormy: rain P.M. 19. Cloudy: thunder and lightning A.M.: rain P.M. 20. Cloudy: rain P.M. 21. Fine. 22. Cloudy. 23. Fine. 24. Cloudy. 25, 26. Fine. 27. Rain. 28. Cloudy 29. Fine: rain with thunder and lightning early A.M. 30. Cloudy. 31. Fine.

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[THIRD SERIES.]

OCTOBER 1834.

XXXV. *A Method of determining the Number of Signals which can be made by the Modern Telegraphs. By* Charles Blackburn, *Esq., A.B.*

To the Editors of the Philosophical Magazine and Journal.

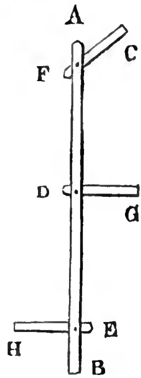
Gentlemen,

ALTHOUGH the French telegraph or semaphore has been in use for a considerable period, I am not aware that any general rule for determining the number of signals that can be made by these instruments has been given to the public. The following investigation, therefore, may not be unacceptable to such of your readers as are interested in that mode of communication. Its object is to furnish a rule for determining the number of distinct signals which can be made by any semaphore, whatever be the number of arms or indicators, or whatever be the number of positions of each arm.

In the Cyclopædia of Rees, the number of signals which the semaphores of the line of communication between Paris and Landau were capable of making, is stated to be 823,543, which is no less than 1,274,608 fewer than the real number, an error not arising from the press, but from the principle of computation. The following method, besides giving the true number of signals, has the advantage of being reducible to an expression of remarkable simplicity.

Problem.—To find the number of signals which can be made by any semaphore having any given number of arms, and each arm taking any given number of positions.

Let AB be an upright pole or staff, carrying any number of arms FC, DG, HE, &c., moveable in a vertical plane about the centres F, D, E, &c., and capable of being placed in any number of positions. This will represent the modern telegraph or semaphore.



Let the number of centres F, D, E, &c. be denoted by c, and the number of positions of each arm by p. Then, since the number of signals that can be made by one arm must be equal to the number of positions of that arm, the number of signals which can be made with one arm $= p$, and the number of signals on the whole, using one arm at a time, will be cp.

Again, since each of the signals which can be made by any one arm, can be repeated with each of the signals of any other arm, it follows that the number which can be made by any two arms together $= p^2$. And since the number of combinations in c things, taken two and two together, $= \frac{c\,.\,\overline{c-1}}{1\,.\,2}$, it follows that the whole number of signals, using two arms at once, $= \frac{c\,.\,\overline{c-1}}{1\,.\,2}\,.\,p^2$.

In like manner it may be shown that the number of signals which can be made using three arms at once $= \frac{c\,.\,\overline{c-1}\,.\,\overline{c-2}}{1\,.\,2\,.\,3}\,p^3$; and that when all the arms are used at once, the number will be $\frac{c\,.\,\overline{c-1}\,.\,\overline{c-2}\,\ldots\ldots\,\overline{c-(c-1)}}{1\,.\,2\,.\,3\,\ldots\ldots\ldots\ldots\,c}\,p^c$, the index of p being always $=$ number of arms used at once. The whole number of signals, therefore, which the telegraph is capable of making will be

$$\left.\begin{array}{r} cp \\ +\dfrac{c\,.\,\overline{c-1}}{1\,.\,2}\,.\,p^2 \\ +\dfrac{c\,.\,\overline{c-1}\,.\,\overline{c-2}}{1\,.\,2\,.\,3}\,.\,p^3 \\ +\ \&c.\quad \&c. \\ +\dfrac{c\,.\,\overline{c-1}\,.\,\overline{c-2}\,\ldots\ldots\,\overline{c-(c-1)}}{1\,.\,2\,.\,3\,\ldots\ldots\ldots\,c}\,.\,p^c \end{array}\right\}$$

But if the binomial $1+p$ be raised to the cth power, it will be found to coincide exactly with the sum of the preceding

terms, with the exception of unity, by which it exceeds it. The expression $(1+p)^c-1$, therefore, will accurately represent the whole number of signals.

Example.—In the Admiralty semaphore, the number of centres or arms $= 2$, and the number of positions of each arm $= 6$. Hence, by the theorem, the number of signals will be $(6+1)^2-1 = 7^2-1 = 48$.

Example 2.—If the number of centres be three, as in the preceding sketch, and the number of positions of each arm be six, the number of signals will be $(6+1)^3-1 = 7^3-1 = 342$.

Example 3.—In the year 1796, a line of telegraphs was established between Paris and Landau, each of which had seven arms, and each arm seven positions. It is required to find the number of signals.

By the theorem, the signals $= (7+1)^7-1 = 8^7-1 =$ 2,097,151.

Corollary.—From the preceding investigation it appears that, for any semaphore having c centres, and each arm p positions, the number of signals which can be made, using one arm at once, will be represented by the second term of the binomial $1+p$ raised to the cth power; the number of signals using two arms at once, by the third term of the same quantity, &c.; and the number of signals, using all the arms at once, by the $(c+1)$th term.

Thus, in the Admiralty semaphore,

The signals using one arm at once $= \quad cp \quad = 2\times 6 = 12$

The signals using two arms at once $= \dfrac{c\,.\,\overline{c-1}}{1\,.\,2}p^2 = 1\times 6^2 = 36$

Total number of signals, as before, $= 48$

In the Paris semaphore before mentioned, the signals using

one arm at once ... ... $= cp = 7\times 7 = 49$

two arms at once ... ... $= \dfrac{c\,.\,\overline{c-1}}{1\,.\,2}p^2 = 21\times 49 = 1029$

three arms at once ... ... $= \dfrac{c\,.\,\overline{c-1}\,.\,\overline{c-2}}{1\,.\,2\,.\,3}p^3 = 35\times 343 = 12005$

four arms ... ... $= \dfrac{c\,.\,\overline{c-1}\,.\,\overline{c-2}\,.\,\overline{c-3}}{1\,.\,2\,.\,3\,.\,4}p^4 = 35\times 2401 = 84035$

five arms ... $= \dfrac{c\,.\,\overline{c-1}\,.\,\overline{c-2}\,.\,\overline{c-3}\,.\,\overline{c-4}}{1\,.\,2\,.\,3\,.\,4\,.\,5}p^5 = 21\times 16807 = 352947$

six arms $= \dfrac{c\,.\,\overline{c-1}\,.\,\overline{c-2}\,.\,\overline{c-3}\,.\,\overline{c-4}\,.\,\overline{c-5}}{1\,.\,2\,.\,3\,.\,4\,.\,5\,.\,6}p^6 = 7\times 117649 = 823543$

seven $= \dfrac{c\,.\,\overline{c-1}\,.\,\overline{c-2}\,.\,\overline{c-3}\,.\,\overline{c-4}\,.\,\overline{c-5}\,.\,\overline{c-6}}{1\,.\,2\,.\,3\,.\,4\,.\,5\,.\,6\,.\,7}p^7 = 1\times 823543 = 823543$

Total number of signals, as before, $=$ 2,097,151

The preceding investigation supposes one arm only on the same centre, as in the modern semaphores; but as they may be made with several arms on the same centre, it may be proper on some future occasion to give the method of finding the signals for such as have any given number of arms on each centre, any number of centres, and any number of positions of each arm; an investigation a little more complicated than the preceding, but, like it, capable of reduction to an expression of great simplicity.

If, in any semaphore, instead of one arm on each centre, we suppose as many arms on each centre as each arm has positions, the number of signals will be $2^{ac}-1$.

Thus, if the Admiralty semaphore had six arms on each centre instead of one, the number of signals would be $2^{2\times 6}-1 = 4095$ instead of 48 as at present.

It is indifferent in the application of the preceding theorem in what order the centres are placed, but they should be in the same vertical plane, and the plane perpendicular to the spectator's line of vision.

Kensington-square, June 3, 1834.

XXXVI. *Remarks on* Mr. Beke's *Papers on the* Gopher-wood, *and the former Extension of the Persian Gulf.* *By* W. G. Carter, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN Mr. Beke's observations in your April Number, on my paper in that for March, that gentleman represents me as stating, that society previously to the Deluge "existed in a state of infancy as regards its culture and knowledge," and "that he apprehends the evidence we possess on the subject ought to lead us to a very different conclusion." Mr. Beke has here mistaken my meaning. I have not expressed any opinion respecting the general culture and knowledge of mankind at that period. My remark was confined to their navigation only. Of all persons, Noah and his family were the least likely to have been ignorant of any art in the building of vessels and boats which was possessed by the antediluvian world. Whatever knowledge they had of the subject, we may infer that they conveyed to their posterity. Yet have we no reason to suppose, from the history of any country, that the arts of ship-building and navigation had made any considerable progress for many ages after the Flood. How, indeed, were they to have been acquired? Navigation is cultivated by an early, very much by any people, in seeking those sup-

plies which the country on their side of the ocean does not afford them, or in emigration. But the nine generations of mankind from Adam to Noah could not have so inconveniently crowded the land they dwelt in, as to set themselves to invent the arts and seek out means for the relief of an overburdened country. Some indeed, without the slightest warrant from the history, neglecting the only criteria we have in the families of Noah who had but three sons, and Shem who had but five, Ham four, and Japheth seven, and adopting a notion which would make the wives of the antediluvian patriarchs mothers of from two to four hundred children each, have filled the whole earth with a swarming population*. But taking the Mosaic history in its common-sense acceptation, mankind was surely then in a state in which "boats and ships (and the use of pitch to cover them) could be very little known."

Mr. Beke appeals to the cognate dialects in support of *kopher* having the meaning of pitch; but unless we are to suppose this manufactured product of a tree was then in use, and obtained a name before the tree whence it was taken, the analogy should have been sought for in the tree itself. No word will, I believe, be found for the pine or fir in any of them (and in Arabic there are several) having any resemblance to *kopher*, which, without the slightest change of letters, is thus made to express both pitch and a tree in the same sentence, and throughout the Hebrew Scriptures is in no other instance employed to signify either.

My remark that the word expressing atonement and mercy-seat were forms of the same word *kopher*, and that if, as in Gen. vi. 14., it expressed pitch, some other word would probably have been *chosen* for those revered objects, with the like import of a covering, Mr. Beke understands to convey, that every root must in every case have attached to it its secondary meaning. Why so? Are a covering, a mercy-seat, and an atonement convertible terms? I still think, in a religious œconomy like that of Moses, in which any association, though often through a very remote idea, with things common, was anxiously excluded, and all the aids, as well of the imagination as of the judgement, were obtained to secure the profound

* Adam seems to have had sons and daughters after Abel, Cain, and Seth (Gen. v. 4.), and all the patriarchs before Noah are said to have had daughters. His are not alluded to. He seems, then, to have had none, and as we know he had but one wife, *he offers the best criterion.* If at each generation the population doubled, that is, if each pair left four children, at the Flood it was under six thousand; if it quadrupled, it was under three millions to people Asia. Whiston, in the way noticed above, makes it 549 thousand millions! (Theory of the Earth, b. iii. ch. 3.)

reverence of the worshipers, some other word would have been selected to express the atonement and mercy-seat, than one to which the people must have been daily familiar in the sense of pitch and a sliming over of pitch, from their traditionary or other history, and that this is one reason, though not "the principal," for inferring that the word, nowhere else used in that sense, is not so used here*.

To the country near Babylon being the spot of Noah's antediluvian residence, Mr. Beke objects, as being founded on mere tradition, mentions the place where the ark rested to be Armenia or the North of Mesopotamia, a statement which can scarcely be considered as founded on anything better,—then, taking that as a fact, observes, "The most philosophical course is to assume that in the neighbourhood where the ark rested there also it was built." Why, in the unequalled atmospheric disturbances which the narrative of the Deluge bespeaks, the ark should be thus immoveable is not very manifest. I assume it was just as likely to be found at length in any other spot, as in the one where such uncertain agency began to act upon it.

It being universally allowed that Babylon (*Babel* in the original) was in the neighbourhood of Hillah, and the Jewish church having furnished us with the same name for both the city and the country of the tower-builders and those of Nimrod †, the natural inference is that they are identical. Mr. Beke, considering that, at this period, the Persian Gulf occupied, or the waters of the rivers desolated, this part, denies their identity, the only reason appearing to be, that at the dispersion in Peleg's days the builders are said to have "left off to build the city." But if we consider that Nimrod was but the third from Noah, and Peleg the fifth, the spots, which in time grew into cities, become but inclosed lands or small villages, and the dominion mere patriarchal rule. On that event happening, enough of the habitations of such a period might have been ready for those who probably remained with Nimrod ‡,

* The very word said to signify pitch is used to designate the Messiah: "Deliver him, &c.: I have found (*kopher*) a ransom."—Job xxxiii. 24.

† See Gen. x. 9. 10. and xi. 2. 9.; 1 Chron. xxxvi. 7.; and Dan. i. 2. By comparing which we find that Babel as well as the Babylon of Nebuchadnezzar was in the land of Shinar.

‡ Is. xxiii. 13. seems distinctly to identify Nimrod's Babel and Babylon: "The Assyrian founded it (Chaldea or Babylon) for them that dwell in the wilderness." That Nimrod was the patriarchal chief of Assyria is determined by Micah v. 6., where Assyria is expressly called "the land of Nimrod." It is surely manifest that it could be his land in no other sense than that he first settled it. Thus the Assyrian Nimrod founded Babel, formed into a social community the remnant of the people scattered and broken

and which must have been provided for the tower-builders, without compelling us to infer that the earth's then scanty inhabitants had given a name to two different places, afterwards, moreover, extended to a third, and all in the same land, signifying "confusion," especially assigned because at one of them "the Lord did confound the language of all the earth." It is for this multiplication that "the warrant" seems required.

Mr. Beke, however, in his paper on the former extension of the Persian Gulf (to which he refers for a further answer to mine), observes, "that if the calculation of Nearchus and the statement of Pliny are to be depended on, the gulf actually occupied the present supposed site of Babylon; so that it was physically impossible for the tower of Babel to be erected at or near" that spot, or of course for Noah to have resided there. The calculation of Nearchus is, that from the gulf to Babylon was 3300 stadia, and the statement of Pliny, that Alexandria, when built by Alexander, on the site of "Charax, was but ten stadia from the sea, while Juba in his time gave it as about 50 miles, and the Arab embassadors and the Roman merchants said it was then about 120." (lib. vi. cap. 27.) This, Mr. Beke considers, seems to give us "the actual rate" of encroachment of the Delta on the gulf during 400 years. But we may go much further, and ascertain, with precision amply sufficient for the present question, the state of circumstances at more than half way, according to common chronology, between our own time and the Deluge. Nearchus, we know, conducted Alexander's fleet up the gulf to the Delta in 325 B.C. Following the course of Nearchus, as given in his own clear account of the voyage preserved by Arrian, from his arrival at the Arosis, the river at the N.E. next before coming to the streams of the Delta, in his progress to Kataderbis and the island of Margastana, in his passage through the channel over the shoals to his arrival at Diridotis (by the Khore Abdallah),

at the dispersion, and the Assyrian of later days "set up the towers and raised up the palaces thereof." To support the present reading, Gen. x. 11., which makes Asshur a man, rather than, as in the margin Targums Onkelos and Babylon, the land Asshur or Assyria, we are to suppose the author of Genesis in relating a pedigree mentioned among the descendants of Ham, one of the descendants of Shem, whose birth he had not yet even noticed, but proceeds afterwards to relate in its proper place and order. And then why was Babel only "the *beginning* of his kingdom," if we are not to understand that it was Nimrod who also "builded Nineveh," &c.? As to the omission of the preposition 'to', "*to* Assyria," it occurs in the Hebrew text so often as to be of little moment either way. See Gen. xxxv. 1. 3. and xlvi. 3.; 1 Sam. x. 8. and xxiii. 4.; 2 Sam. vi. 10. 12. and x. 2. &c.

on the S.W. side of the Delta, and comparing it with the present state of the country, we learn with surprise the small degree of change which the general characters of the coast have undergone during the lapse of so many ages. Dr. Vincent, in his able work "On the Commerce and Navigation of the Ancients in the Indian Ocean," adverting to this remarkable fact, observes, that Capt. Howe's chart "explains the journal of Nearchus as perfectly as if it had been composed by a person on board of his fleet," (vol. i. p. 423.), and (p. 466.) "The pilot on board Nearchus's ship steered exactly the same course" (*along the coast of the Delta,*) "as Mr. Cluer's Karack pilot 2000 years afterwards." The junction of the river called by Arrian the Eulæus (coming from the N. or N.E.) with the Tigris by the still existing ancient Hoffar canal, across which Alexander sent a part of his fleet while he sailed down the Eulæus to the mouths of the Tigris, and so round to meet it, (Arrian, Exp. Alex. vii. 7.) further shows that to the point in question any later encroachments on the gulf must be very unimportant.

This supposed extension of the Delta over nearly 400 miles seems, moreover, as little aided by Nearchus's calculation of the distance in his time between the gulf and Babylon. Dr. Vincent intimates that in calling it 3300 stadia, Nearchus should be understood as speaking of stadia of about eight to a mile (412 miles). To this Mr. Beke objects as merely an estimate of convenience. Now it happens that Pliny, the other authority, who must have understood Nearchus's terms of distance better than we can, expressly says, (vi. 26.) "Euphrate navigari Babylonem e Persico mari 412 mill. pass." (about 412 miles) "tradunt Nearchus et Onesicritus." The latter was pilot and master of Alexander's own ship. Then as to Juba's opinion Pliny goes on to say, "Juba a Babylone Characem 175 mill. pass.;" that is, Juba (the second) made it in his time, adding the 50 miles he accounted it from Charax to the sea, 225 miles only from the sea to Babylon, and this was 340 years *after* the calculation of Nearchus and Onesicritus which made it 412. So that we shall, perhaps, be inclined to concur with Pliny in the remark, "Inconstantiam mensuræ diversitas auctorum facit." From such discordant opinions it is hard to "learn the true measure," rather than infer from them "the actual rate at which the Persian Gulf had been filled up during the 400 years immediately preceding his time."

Mr. Beke animadverts on Mr. Lyell's speaking of the union of the Euphrates and Tigris as if not manifestly within the historic æra (in Geology, vol. i.), seeing that Pliny (vi. 27.) says that in ancient times some made it 25 miles between their

mouths, and some 7. But it appears from Pliny's account, that long before his time they had united above the embouchure *somewhere*, not, as Mr. Beke's paper implies, by encroachments on the gulf and formation of delta, but simply by the labour of hands; for Pliny immediately adds, "sed longo tempore Euphratem præclusere Orcheni et accolæ agros rigantes nec nisi Pasitigris defertur in mare." And that the stream thus cut off was only one of the streams of the Delta called in the country, as Dr. Vincent says it is to this day, the mouth of the Euphrates, and that these rivers had a higher junction, seems to be the only inference from other ancient authorities, as Arrian, who, while in his Expedition of Alexander he says, "the Euphrates disappears in the marshes," and again, "the Tigris falls into the Persian Gulf;...where the Euphrates terminates there is not much water, it is marshy, and its course is staid *," in his Voyage of Nearchus makes him arrive at its mouth, giving it no doubt its local appellation given it by his narrator. Pliny, whose account of the two rivers is not very explicable, adds to the opinions of Juba, &c.: "Some state, that beyond Babylon the Euphrates flows in one channel for 87 miles before it is drawn off into several courses on the confines of Charax †." Taking this to be the Tigris, and connecting it with his notion about the disappearance of the Euphrates, we obtain an intimation of their junction at Khorna, where they now meet, more than 100 miles inland. Strabo observes, that Eratosthenes (about 500 B. C.) had spoken of the figure "which the Tigris and Euphrates make falling together into one;" and Onesicritus, of both rivers as flowing into a lake, and the Euphrates as flowing out of it to the sea ‡. Mela says, that "the Euphrates does not continue to the ocean...ceases in a slender stream, and has no mouth §." Ptolemy, although he repeatedly mentions the mouths of the Tigris, has noticed none to the Euphrates, and appears to refer to their junction as far inland ‖; while Justin states that "the Tigris is received into the marshes of the

* *Εις τεναγη ὁ Ευφρατης αφανιζεται.*—*Arrian, Exp. Alex.*, lib. v. cap. 4. Ὁ *Τιγρις...εσβαλλει ες τον ποντον τον Περσικον ου πολυ ὑδωρ ὁ Ευφρατης τελευτων και τεναγωδης ες τϐτο ϐτως αποπαυεται.*—*Ibid.* lib. vii. cap. 7.

† "Fluere, aliqui, ultra Babylonem continuo alveo priusquam distrahitur ad rigua 87 mill. pass. ubi desinit alveo munire ad confinium Characis accedente tractu."—*Pliny, Nat. Hist.*, lib. vi. cap. 26.

‡ Ὁ *ποιϐσι συμπιπτουτες εις ἑν ὁ τε Τιγρις και ὁ Ευφρατης...ὡς φησι.*—*Strabo*, lib. ii. p. 79. et p. 729.

§ "Euphrates non perdurat in pelagus...tenuis rivus despectus emoritur, et nusquam manifesto exitu effluit sed deficit."—*Mela*, lib. iii. cap. 8.

‖ Ptolemy, p. 145—149.

Euphrates*." What the single stream to the Delta was called, whether by the name of the Tigris or of the Euphrates, does not seem very important; but notwithstanding some discrepancies, the conclusion from the above authorities surely is, that they have at a very early period, and probably always, united inland somewhere. Dr. Vincent (i. 422.) says, "Khorna, I am persuaded, was the grand confluence in all ages." But the large and numerous canals cut in very early times about this country, combined with the many streams of the Delta, have given rise to misconceptions as to the true course of the parent rivers. These streams offer the natural indications of the commencement of the Delta below Bosra, (or Bossora, about 70 miles from the sea,) where, as usual, from the want of slope and momentum, the water begins to multiply its channels.

If, then, the situation of the Delta, during more than one half of the interval between our time and the Flood, afford any illustration of its state during the other, the presence of the gulf at 300 or 400 miles from its present limit did not preclude the residence of Noah in that vicinity. But Mr. Beke, quoting from Mr. Rich's first memorial on Babylon a statement of an extensive inundation of the Euphrates (continuing for three or four months of the year), infers that the country in the neighbourhood of Babylon, if not occupied by the gulf, must at the time of the building of Babel have been "unfit for human habitation," from its flooded condition. That, however, does not appear to be a necessary consequence. It should seem that there was less rain, and that the waters were not in such excess at an early period (Polyclites ap. Strab. 742.; Arrian, Ex. Alex. vii. 21.; Herod. Clio 179.; Mela, i. 11.);—and the objection would equally apply to the other postdiluvian

* "Tigris in paludes Euphratis recipitur."—*Justin.* lib. xlii. cap. 3. Though Strabo's report of Eratosthenes is of an actual junction of the rivers, a falling *together* into one, he says of both Tigris and Euphrates, it goes to the sea. (lib. xii. 521. and xi. 522.) Herodotus says the same (Clio 20. Erato 180.); and Arrian, we see, though he speaks of the Euphrates disappearing in the lakes or marshes, mentions its mouth. Arrian is above explained. The other statements are true now, that is, both rivers being in the same channel till the waters begin to diverge at Bosra, then passing on partly by the canal to the Khore Abdallah, and partly by the channel to the Delta. Ptolemy's eastern and western mouth of the Tigris indicate exactly the present state of the river. Thus Eratosthenes, Arrian, Mela, Justin, Ptolemy, Pliny, all understood that the distinct course of one river failed. A river into a lake may be considered either as terminated there, or continued by a stream issuing at the other end. There is no necessary disagreement of any of them with Nearchus, and if there had been, it could not compete with the plain details of his close personal observation.

settlements, as the cities of the plain, Egypt, and especially Nineveh,—contemporaneous with Babel, in "the lowlands of the Tigris," a valley 8 or 10 miles broad, and where the floods were so great that "of old it was like a pool of water." (Nahum ii. 8.).

In my paper I referred to the productions of "the climate" only where the ark was built, not to the particular country,—to the vicinity of Babylon,—as Mr. Beke has taken it. But place the abode of Noah in any of the spots that have been assigned for the residence of the antediluvian race, in either Mesopotamia or Palestine, we have still together pits of native bitumen and a country propitious to the cypress. Diod. Sicu. (xix. 702.) states that Antigonus (about 313 B.C.) procured supplies of cypress-trees from Lebanon, "*θαυμαζων το τε καλος και μεγεθος*," of surprising magnitude and beauty. Baronius, Ann. Eccles. ad A. 714., mentions "the fleet of the Saracens hastening from Alexandria to Phœnicia to cut cypress-trees;" in both instances for the purpose of ship-building: and see Gen. xiv. 10. As to the country of Babylon, its dry soil appears to have been most propitious to this tree, and to *none other fitted for that use.* We learn from Diod. Sicu. (lib. ii. c. 1.) that in the ancient bridge at Babylon, said to have been built by Semiramis, cypress-wood was employed. And Arrian* reports of Aristobulus, who accompanied Alexander the Great: "He says that Alexander had another fleet, built of the cypresses cut in Babylonia, for of these trees *alone* there was an abundant supply, the country of Assyria being poor in other timber fit for ship-building." The objection, then, to the cypress being the *gopher*-wood of the ark, because not yielded by the country where the ark was built, is not very forcible.

But even were it admitted to be true that the site of ancient Babylon and all the lands S.E. of it to the sea had been formerly in the gulf, or had been otherwise covered with water, it would not show that "the ark could not possibly have been built anywhere in the neighbourhood" alluded to by me, even taking it to be on that side of the Euphrates, for my allusion was to the part where the bitumen is produced from the earth. It is not ascertained that bitumen in any quantity was ever found nearer to Babylon than Is or Heet. Herodotus (Clio 179.) expressly states that it came thence for

* *Λεγει ότι και αλλος αυτω εναυπηγειτο στολος τεμνοντι τας κυπαρισσους τας εν τη Βαβυλωνια τουτων γαρ μονων των δενδρων ευποριαν ειναι εν τη χωρα των Ασσυριων των δε αλλων όσα ες ναυπηγιαν απορως εχειν την γην ταυτην.* —*Arrian, Exp. Alex.*, lib. vii. cap. 19.

the buildings of the city. But Heet is 128 miles to the N.W. of it, and from the intervening country have been obtained the lime and gypsum employed on them. (Rich, i. 64.; Buckingham, 453.) The Delta surely did not extend to Heet. But if Heet had not existence or were inaccessible about the birth of Peleg, will the same be said of Kerkook, where are the next nearest pools of bitumen, (Rich, i. Memor. 63.) about 196 miles in a line to the N. and beyond two ridges of mountains (Buckingham, 335, &c.)? or again, of Arbela, about 46 such miles further north*, near which place, says Strabo, (lib. vi. 741.) there is a fountain of naphtha, and not noticing any other product, mentions with this bituminous fountain a cypress grove.

Upon the whole, then, whether we place the abode of Noah in the southern part of Mesopotamia or Palestine, in either of the parts which have been assigned for it there was bitumen if it were needed: a motive at that age for seeking a substitute: "pitch trees" would thus be unknown. The cypress, of all woods the most suitable, was afforded for his ark; in particular, Babylonia seems to have offered none other; the name suggests it: and then, if the ark must have remained in the same place throughout the cataclysm, and was built where that left it, even the site of Babylon might have been the spot, for any impediment the waters of the gulf could offer to the builder or the waters of the rivers, that, as to them, would not equally apply to the settlements of Nimrod, which, like Nineveh, grew to be cities, for their place is, and especially in the East, where Nineveh was, by the side of a river.

I am, yours, &c.

May 1834. W. G. CARTER.

XXXVII. *Experimental Researches in Electricity.—Seventh Series.* By MICHAEL FARADAY, *D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c.*

[Continued from p. 181.]

¶ vi. *On the primary or secondary Character of the Bodies evolved at the Electrodes.*

742. BEFORE the *volta-electrometer* could be employed in determining, as a *general law*, the constancy of electro-decomposition, it became necessary to examine a di-

* D'Anville, indeed, makes Arbela due E. from Mousul, and equidistant with it from the gulf.

stinction, already recognised among scientific men, relative to the products of that action, namely, their primitive or secondary character; and, if possible, by some general rule or principle, to decide when they were of the one or the other kind. It will appear hereafter that great mistakes respecting electro-chemical action and its consequences, have arisen from confounding these two classes of results together.

743. When a substance under decomposition yields at the electrodes those bodies uncombined and unaltered which the electric current has separated, then they may be considered as primary results, even though themselves compounds. Thus the oxygen and hydrogen from water are primary results; and so also are the acid and alkali (themselves compound bodies) evolved from sulphate of soda. But when the substances separated by the current are changed at the electrodes before their appearance, then they give rise to secondary results, although in many cases the bodies evolved are elementary.

744. These secondary results occur in two ways, being sometimes due to the mutual action of the evolving substance and the matter of the electrode, and sometimes to its action upon the substances contained in the decomposing conductor itself. Thus, when carbon is made the positive electrode in dilute sulphuric acid, carbonic oxide and carbonic acid appear there instead of oxygen; for the latter, acting upon the matter of the electrode, produces these secondary results. Or if the positive electrode, in a solution of nitrate or acetate of lead, be platina, then peroxide of lead appears there, equally a secondary result with the former, but now depending upon an action of the oxygen on a substance in the solution. Again, when ammonia is decomposed by platina electrodes, nitrogen appears at the *anode* *; but though an *elementary* body, it is a *secondary* result in this case, being derived from the chemical action of the oxygen electrically evolved there, upon the ammonia in the surrounding solution (554.). In the same manner when aqueous solutions of metallic salts are decomposed by the current, the metals evolved at the *cathode*, though elements, are *always* secondary results, and not immediate consequences of the decomposing power of the electric current.

745. Many of these secondary results are extremely valuable; for instance, all the interesting compounds which M. Becquerel has obtained by feeble electric currents are of this nature; but they are essentially chemical, and must, in the theory of electrolytic action, be carefully distinguished from

* *Annales de Chimie*, 1804, tom. li. p. 167.

those which are directly due the action of the electric current.

746. The nature of the substances evolved will often lead to a correct judgement of their primary or secondary character, but is not sufficient alone to establish that point. Thus, nitrogen is said to be attracted sometimes by the positive and sometimes by the negative electrode, according to the bodies with which it may be combined (554. 555.), and it is on such occasions evidently viewed as a primary result*; but I think I shall show, that, when it appears at the positive electrode, or rather at the *anode*, it is a secondary result (748.). Thus, also, Sir Humphry Davy†, and with him the great body of chemical philosophers, (including myself,) have given the appearance of copper, lead, tin, silver, gold, &c., at the negative electrode, when their aqueous solutions were acted upon by the voltaic current, as proofs that the metals, as a class, were attracted to that surface; thus assuming the metal in each case to be a primary result. These however, I expect to prove, are all secondary results; the mere consequence of chemical action, and no proofs of the attraction or the law announced ‡.

747. But when we take to our assistance the law of *constant electro-chemical action* already proved with regard to water (732.), and which I hope to extend satisfactorily to all bodies (821.), and consider the *quantities* as well as the *nature* of the substances set free, a generally accurate judgement of the primary or secondary character of the results may be formed: and this important point, so essential to the theory of electro-decomposition, since it decides what are the particles directly under the influence of the current, (distinguishing them from such as are not affected,) and what are the results to be expected, may be established with such degree of certainty as to remove innumerable ambiguities and doubtful considerations from this branch of the science.

748. Let us apply these principles to the case of ammonia, and the supposed determination of nitrogen to one or the other *electrode* (554. 555.). A pure strong solution of ammonia is as bad a conductor, and therefore as little liable to electro-

* *Annales de Chimie*, 1804, tom. li. p. 172.

† Elements of Chemical Philosophy, pp. 144, 161.

‡ It is remarkable that up to 1804 it was the received opinion that the metals were reduced by the nascent hydrogen. At that date the general opinion was reversed by Hisinger and Berzelius (*Annales de Chimie*, 1804, tom. li. p. 174.), who stated that the metals were evolved directly by the electricity; in which opinion it appears, from that time, Davy coincided (Philosophical Transactions, 1826, p. 388.); [or Phil. Mag. and Annals, N.S. vol. i. p. 35.—Edit.]

decomposition, as pure water; but when sulphate of ammonia is dissolved in it, the whole becomes a conductor; nitrogen *almost* and occasionally *quite* pure is evolved at the *anode*, and hydrogen at the *cathode*; the ratio of the volume of the former to that of the latter varying, but being as 1 to about 3 or 4. This result would seem at first to imply that the electric current had decomposed ammonia, and that the nitrogen had been determined towards the positive electrode. But when the electricity used was measured out by the volta-electrometer (707. 736.), it was found that the hydrogen obtained was exactly in the proportion which would have been supplied by decomposed water, whilst the nitrogen had no certain or constant relation whatever. When, upon multiplying experiments, it was found that, by using a stronger or weaker solution, or a more or less powerful battery, the gas evolved at the *anode* was a mixture of oxygen and nitrogen, varying both in proportion and absolute quantity, whilst the hydrogen at the *cathode* remained constant, no doubt could be entertained that the nitrogen at the *anode* was a secondary result, depending upon the chemical action of the nascent oxygen, determined to that surface by the electric current, upon the ammonia in solution. It was the water, therefore, which was electrolyzed, not the ammonia. Further, the experiment gives no real indication of the tendency of the element nitrogen to either one electrode or the other; nor do I know of any experiment with nitric acid, or other compounds of nitrogen, which shows the tendency of this element, under the influence of the electric current, to pass in either direction along its course.

749. As another illustration of secondary results, the effects on a solution of acetate of potassa may be quoted. When a very strong solution was used, more gas was evolved at the *anode* than at the *cathode*, in the proportion of 4 to 3 nearly: that from the *anode* was a mixture of carbonic oxide and carbonic acid; that from the *cathode* pure hydrogen. When a much weaker solution was used, less gas was evolved at the *anode* than at the *cathode*; and it now contained carburetted hydrogen, as well as carbonic oxide and carbonic acid. This result of carburetted hydrogen at the positive electrode has a very anomalous appearance, if considered as an immediate consequence of the decomposing power of the current. It, however, as well as the carbonic oxide and acid, is only a *secondary result*; for it is the water alone which suffers electro-decomposition, and it is the oxygen eliminated at the *anode* which, reacting on the acetic acid, in the midst of which it is evolved, produces those substances that finally appear there. This is fully proved by experiments with the volta-electrome-

ter (707.); for then the hydrogen evolved from the acetate at the *cathode* is always found to be definite, being exactly proportionate to the electricity which has passed through the solution, and, in quantity, the same as the hydrogen evolved in the volta-electrometer itself. The appearance of the carbon in combination with the hydrogen at the positive electrode, and its non-appearance at the negative electrode, are in curious contrast with the results which might have been expected from the law usually accepted respecting the final places of the elements.

750. If the salt in solution be an acetate of lead, then the results at both electrodes are secondary, and cannot be used to estimate or express the amount of electro-chemical action, except by a circuitous process (843.). In place of oxygen, or even the gases already described (749.), peroxide of lead now appears at the positive, and lead itself at the negative electrode. When other metallic solutions are used, containing, for instance, peroxides, as that of copper, combined with this or any other decomposable acid, still more complicated results will be obtained; which, viewed as direct results of the electrochemical action, will, in their proportions, present nothing but confusion, but will appear perfectly harmonious and simple if they be considered as secondary results, and will accord in their proportions with the oxygen and hydrogen evolved from water by the action of a definite quantity of electricity.

751. I have experimented upon many bodies, with a view to determine whether the results were primary or secondary. I have been surprised to find how many of them, in ordinary cases, are of the latter class, and how frequently water is the only body electrolyzed in instances where other substances have been supposed to give way. Some of these results I will give in as few words as possible.

752. *Nitric Acid.*—When very strong, it conducted well, and yielded oxygen at the positive electrode. No gas appeared at the negative electrode; but nitrous acid, and apparently nitric oxide, were formed there, which, dissolving, rendered the acid yellow or red, and at last even effervescent, from the spontaneous separation of nitric oxide. Upon diluting the acid with its bulk or more of water, gas appeared at the negative electrode. Its quantity could be varied by variations, either in the strength of the acid or of the voltaic current: for that acid from which no gas separated at the *cathode*, with a weak voltaic battery, did evolve gas there with a stronger; and that battery which evolved no gas there, with a strong acid, did cause its evolution with an acid more dilute. The gas at the *anode* was always oxygen; that at the *cathode* hy-

drogen. When the quantity of products was examined by the volta-electrometer (707.), the oxygen, whether from strong or weak acid, proved to be in the same proportion as from water. When the acid was diluted to specific gravity 1·24, or less, the hydrogen also proved to be the same in quantity as from water. Hence I conclude that the nitric acid does not undergo electro-chemical decomposition, but the water only; that the oxygen at the *anode* is always a primary result, but that the products at the *cathode* are often secondary, and due to the reaction of the hydrogen upon the nitric acid.

753. *Nitre.*—A solution of this salt yields very variable results, according as one or other form of tube is used, or as the electrodes are large or small. Sometimes the whole of the hydrogen of the water decomposed may be obtained at the negative electrode; at other times, only a part of it, because of the ready formation of secondary results. The solution is a very excellent conductor of electricity.

754. *Nitrate of ammonia*, in aqueous solution, gives rise to secondary results very varied and uncertain in their proportions.

755. *Sulphurous Acid.*—Pure liquid sulphurous acid does not conduct nor suffer decomposition by the voltaic current*, but, when dissolved in water, the solution acquires conducting power, and is decomposed, yielding oxygen at the *anode*, and hydrogen and sulphur at the *cathode*.

756. A solution containing sulphuric acid in addition, was a better conductor. It gave very little gas at either electrode: that at the *anode* was oxygen, that at the *cathode* pure hydrogen. From the *cathode* also rose a white turbid stream, consisting of diffused sulphur, which soon rendered the whole solution milky. The volumes of gases were in no regular proportion to the quantities evolved from water in the volta-electrometer. I conclude that the sulphurous acid was not at all affected by the electric current in any of these cases, and that the water present was the only body electro-chemically decomposed; that, at the *anode*, the oxygen from the water converted the sulphurous acid into sulphuric acid, and, at the *cathode*, the hydrogen electrically evolved decomposed the sulphurous acid, combining with its oxygen, and setting its sulphur free. I conclude that the sulphur at the negative electrode was only a secondary result; and, in fact, no part of it was found combined with the small portion of hydrogen

* See also De la Rive, *Bibliothèque Universelle*, tom. xl. p. 205; or Quarterly Journal of Science, vol. xxvii. p. 407.

which escaped when weak solutions of sulphurous acid were used.

757. *Sulphuric Acid.*—I have already given my reasons for concluding that sulphuric acid is not electrolyzable, *i. e.* not decomposable directly by the electric current, but occasionally suffering by a secondary action at the *cathode* from the hydrogen evolved there (681.). In the year 1800, Davy considered the sulphur from sulphuric acid as the result of the action of the nascent hydrogen*. In 1804, Hisinger and Berzelius stated that it was the direct result of the action of the voltaic pile†; an opinion which from that time Davy seems to have adopted, and which has since been commonly received by all. The change of my own opinion requires that I should correct what I have already said of the decomposition of sulphuric acid in a former series of these Researches (552.): I do not now think that the appearance of the sulphur at the negative electrode is an immediate consequence of electrolytic action.

758. *Muriatic Acid.*—A strong solution gave hydrogen at the negative electrode, and chlorine only at the positive electrode; of the latter, a part acted on the platina and a part was dissolved. A minute bubble of gas remained; it was not oxygen, but probably air previously held in solution.

759. It was an important matter to determine whether the chlorine was a primary result, or only a secondary product, due to the action of the oxygen evolved from water at the *anode* upon the muriatic acid; *i. e.* whether the muriatic acid was electrolyzable, and if so, whether the decomposition was *definite.*

760. The muriatic acid was gradually diluted. One part with six of water gave only chlorine at the *anode.* One part with eight of water gave only chlorine; with nine of water, a little oxygen appeared with the chlorine: but the occurrence or non-occurrence of oxygen at these strengths depended, in part, on the strength of the voltaic battery used. With fifteen parts of water, a little oxygen, with much chlorine, was evolved at the *anode.* As the solution was now becoming a bad conductor of electricity, sulphuric acid was added to it: this caused more ready decomposition, but did not sensibly alter the proportion of chlorine and oxygen.

761. The muriatic acid was now diluted with 100 times its volume of dilute sulphuric acid. It still gave a large proportion of chlorine at the *anode*, mingled with oxygen; and the result was the same, whether a voltaic battery of 40 pairs of

* Nicholson's Quarterly Journal, vol. iv. pp. 280, 281.

† *Annales de Chimie,* 1804, tom. li. p. 173.

plates or one containing only 5 pairs were used. With acid of this strength, the oxygen evolved at the *anode* was to the hydrogen at the *cathode*, in volume, as 17 is to 64; and therefore the chlorine would have been 30 volumes, had it not been dissolved by the fluid.

762. Next, with respect to the quantity of elements evolved. On using the volta-electrometer, it was found that, whether the strongest or the weakest muriatic acid were used, whether chlorine alone or chlorine mingled with oxygen appeared at the *anode*, still the hydrogen evolved at the *cathode* was a constant quantity, *i. e.* exactly the *same* as the hydrogen which the *same quantity of electricity* could evolve from water.

763. This constancy does not decide whether the muriatic acid is electrolyzed or not, although it proves that if so, it must be in definite proportions to the quantity of electricity used. Other considerations may, however, be allowed to decide the point. The analogy between chlorine and oxygen, in their relations to hydrogen, is so strong, as to lead almost to the certainty, that, when combined with that element, they would perform similar parts in the process of electro-decomposition. They both unite with it in single proportional or equivalent quantities; and, the number of proportionals appearing to have an intimate and important relation to the decomposability of a body (697.), those in muriatic acid, as well as in water, are the most favourable, or those, perhaps even necessary to decomposition. In other binary compounds of chlorine also, where nothing equivocal depending on the simultaneous presence of it and oxygen is involved, the chlorine is directly eliminated at the *anode* by the electric current. Such is the case with the chloride of lead (395.), which may be justly compared with protoxide of lead (402.), and stands in the same relation to it as muriatic acid to water. The chlorides of potassium, sodium, barium, &c., are in the same relation to the protoxides of the same metals, and present the same results under the influence of the electric current (402.).

764. From all the experiments, combined with these considerations, I conclude that muriatic acid is decomposed by the direct influence of the electric current, and that the quantities evolved are, and therefore the chemical action is, *definite for a definite quantity of electricity*. For though I have not collected and measured the chlorine, in its separate state, at the *anode*, there can exist no doubt as to its being proportional to the hydrogen at the *cathode*; and the results are therefore sufficient to establish the general law of *constant electro-chemical action* in the case of muriatic acid.

765. In the dilute acid (761.), I conclude that a part of the

water is electro-chemically decomposed, giving origin to the oxygen, which appears mingled with the chlorine at the *anode*. The oxygen *may* be viewed as a secondary result; but I incline to believe that it is not so: for, if it were, it might be expected in largest proportion from the stronger acid, whereas the reverse is the fact. This consideration, with others, also leads me to conclude that muriatic acid is more easily decomposed by the electric current than water; since, even when diluted with eight or nine times its quantity of the latter fluid, it alone gives way, the water remaining unaffected.

766. *Chlorides.*—On using solutions of chlorides in water, —for instance, the chlorides of sodium or calcium,—there was evolution of chlorine only at the positive electrode, and of hydrogen, with the oxide of the base, as soda or lime, at the negative electrode. The process of decomposition may be viewed as proceeding in two or three ways, all terminating in the same results. Perhaps the simplest is to consider the chloride as the substance electrolyzed, its chlorine being determined to and evolved at the *anode*, and its metal passing to the *cathode*, where, finding no more chlorine, it acts upon the water, producing hydrogen and an oxide as secondary results. As the discussion would detain me from more important matter, and is not of immediate consequence, I shall defer it for the present. It is, however, of *great consequence* to state, that, on using the volta-electrometer, the hydrogen in both cases was definite; and if the results do not prove the definite decomposition of chlorides, (which shall be proved elsewhere,—789. 794. 814.) they are not in the slightest degree opposed to such a conclusion, and *do* support the *general law*.

767. *Hydriodic Acid.*—A solution of hydriodic acid was affected exactly in the same manner as muriatic acid. When strong, hydrogen was evolved at the negative electrode, in definite proportion to the quantity of electricity which had passed, *i. e.* in the same proportion as was evolved by the same current from water; and iodine without any oxygen was evolved at the positive electrode. But when diluted, small quantities of oxygen appeared with the iodine at the *anode*, the proportion of hydrogen at the *cathode* remaining undisturbed.

768. I believe the decomposition of the hydriodic acid in this case to be direct, for the reasons already given respecting muriatic acid (763. 764.).

769. *Iodides.*—A solution of iodide of potassium being subjected to the voltaic current, iodine appeared at the positive electrode (without any oxygen), and hydrogen with free alkali at the negative electrode. The same observations as to the

mode of decomposition are applicable here as were made in relation to the chlorides when in solution (766.).

770. *Hydro-fluoric Acid and Fluorides.*—Solution of hydrofluoric acid did not appear to be decomposed under the influence of the electric current: it was the water which gave way apparently. The fused fluorides were electrolyzed (417.); but having during these actions obtained *fluorine* in the separate state, I think it better to refer to a future series of these Researches, in which I purpose giving a fuller account of the results than would be consistent with propriety here.

771. *Hydro-cyanic acid* in solution conducts very badly. The definite proportion of hydrogen (equal to that from water) was set free at the *cathode*, whilst at the *anode* a small quantity of oxygen was evolved and apparently a solution of cyanogen formed. The action altogether corresponded with that on a dilute muriatic or hydriodic acid. When the hydrocyanic acid was made a better conductor by sulphuric acid, the same results occurred.

Cyanides.—With a solution of the cyanide of potassium, the result was precisely the same as with a chloride or iodide. No oxygen was evolved at the positive electrode, but a brown solution formed there. For the reasons given when speaking of the chlorides (766.), and because a fused cyanide of potassium evolves cyanogen at the positive electrode*, I incline to believe that the cyanide in solution is *directly* decomposed.

772. *Ferro-cyanic acid* and the *ferro-cyanides*, as also *sulpho-cyanic acid* and the *sulpho-cyanides*, presented results corresponding with those just described (771.).

773. *Acetic Acid*—Glacial acetic acid, when fused (405.), is not decomposed by, nor does it conduct, electricity. On adding a little water to it, still there were no signs of action; on adding more water, it acted slowly and about as water alone would do. Dilute sulphuric acid was added to it in order to make it a better conductor; then the definite proportion of hydrogen was evolved at the *cathode*, and a mixture of oxygen in very deficient quantity, with carbonic acid, and a little carbonic oxide, at the *anode*. Hence it appears that acetic acid is not electrolyzable, but that a portion of it is decomposed by the oxygen evolved at the *anode*, producing secondary results, varying with the strength of the acid, the intensity of the current, and other circumstances.

774. *Acetates.*—One of these has been referred to already,

* It is a very remarkable thing to see carbon and nitrogen in this case determined powerfully towards the positive surface of the voltaic battery; but it is perfectly in harmony with the theory of electro-chemical decomposition which I have advanced.

as affording only secondary results relative to the acetic acid (749.). With many of the metallic acetates the results at both electrodes are secondary (746. 750.).

Acetate of soda fused and anhydrous is directly decomposed, being, as I believe, a true electrolyte, and evolving soda and acetic acid at the *cathode* and *anode*. These, however, have no sensible duration, but are immediately resolved into other substances; charcoal, sodiuretted hydrogen, &c., being set free at the former, and as far as I could judge under the circumstances, acetic acid mingled with carbonic oxide, carbonic acid, &c., at the latter.

775. *Tartaric Acid.*—Pure solution of tartaric acid is almost as bad a conductor as pure water. On adding sulphuric acid to it, it conducted well, the results at the positive electrode being primary or secondary in different proportions, according to variations in the strength of the acid and the power of the electric current (752.). Alkaline tartrates gave a large proportion of secondary results at the positive electrode. The hydrogen at the negative electrode remained constant unless certain metallic salts were used.

776. Solutions of salts containing other vegetable acids, as the benzoates; of sugar, gum, &c., dissolved in dilute sulphuric acid; of resin, albumen, &c., dissolved in alkalies, were in turn submitted to the electrolytic power of the voltaic current. In all these cases, secondary results to a greater or smaller extent were produced at the positive electrode.

777. In concluding this division of these Researches, it cannot but occur to the mind that the final result of the action of the electric current upon substances placed between the electrodes, instead of being simple may be very complicated. There are two modes by which these substances may be decomposed, either by the direct force of the electric current, or by the action of bodies which that current may evolve. There are also two modes by which new compounds may be formed, *i. e.* by combination of the evolving substances whilst in their nascent state (658.), directly with the matter of the electrode; or else their combination with those bodies, which being contained in, or associated with, the decomposing conductor, are necessarily present at the *anode* and *cathode*. The complexity is rendered still greater by the circumstance that two or more of these actions may occur simultaneously, and also in variable proportions to each other. But it may in a great measure be resolved by attention to the principles already laid down (747.).

778. When *aqueous* solutions of bodies are used, secondary results are exceedingly frequent. Even when the water is not

present in large quantity, but is merely that of combination, still secondary results often ensue: for instance, it is very possible that in Sir Humphry Davy's decomposition of the hydrates of potassa and soda, a part of the potassium produced was the result of a secondary action. Hence, also, a frequent cause for the disappearance of the oxygen and hydrogen which would otherwise be evolved: and when hydrogen does *not* appear at the *cathode* in an *aqueous solution*, it perhaps always indicates that a secondary action has taken place there. No exception to this rule has as yet occurred to my observation.

779. Secondary actions are *not confined to aqueous solutions*, or cases where water is present. For instance, various chlorides acted upon, when fused (402.), by platina electrodes, have the chlorine determined electrically to the *anode.* In many cases, as with the chlorides of lead, potassium, barium, &c., the chlorine acts on the platina and forms a compound with it, which dissolves; but when protochloride of tin is used, the chlorine at the *anode* does not act upon the platina, but upon the chloride already there, forming a perchloride which rises in vapour (790. 804.). These are, therefore, instances of secondary actions of both kinds, produced in bodies containing no water.

780. The production of boron from fused borax (402. 417.), is also a case of secondary action; for boracic acid is not decomposable by electricity (408.), and it was the sodium evolved at the *cathode* which, reacting on the boracic acid around it, took oxygen from it and set boron free in the experiments formerly described.

781. Secondary actions have already, in the hands of M. Becquerel, produced many interesting results in the formation of compounds; some of them new, others imitations of those occurring naturally*. It is probable they may prove equally interesting in an opposite direction, *i. e.* as affording cases of analytic decomposition. Much information regarding the composition, and perhaps even the arrangement of the particles of such bodies as the vegetable acids and alkalies, and organic compounds generally, will probably be obtained by submitting them to the action of nascent oxygen, hydrogen, chlorine, &c., at the electrodes; and the action seems the more promising, because of the thorough command which we possess over attendant circumstances, such as the strength of the current, the size of the electrodes, the nature of the decomposing conductor, its strength, &c., all of which may be

§ *Annales de Chimie,* tom. xxxv. p. 113.

expected to have their corresponding influence upon the final result.

782. It is to me a great satisfaction that the extreme variety of secondary results have presented nothing opposed to the doctrine of a constant and definite electro-chemical action, to the particular consideration of which I shall now proceed.

[To be continued.]

XXXVIII. *On the Tides in the Bay of Morecambe.* By JOHN NIXON, *Esq.**

THE beautiful bay of Morecambe extends from the Irish Sea seventeen miles in a north-easterly direction. From the entrance (between Rossal Point and Walney Island) to Humphrey Point, a distance of fourteen miles, beyond which the bay contracts into two narrow arms, the width varies from eleven to fifteen miles.

At the mouth of the bay, situate some little distance below the parallel of latitude wherein the tidal current from St. George's Channel meets that from the North Channel, we have shallows to the north-west, and to the south-east deepening water up to the margin of the sands by the shore. Off Rossal Point the soundings at low water are nearly thirty fathoms, but the depth decreases gradually up the same side of the bay, and towards the opposite shore, as far as to Poulton (a distance of twelve miles north-east) where the sea ebbs out, or nearly so, at high tides. From about Poulton draw a line in a direction to bisect the entrance to the bay, and we shall have, to the right, sands either laid bare, or only one to three fathoms under the sea at low-water spring tides. The Grange and Furness channels range by opposite shores the length of these sands, the former conveying at low water to the open sea the Kent, Winster, and Keer, and the latter the waters from Windermere and Coniston lakes, &c. After heavy rains the swoln rivers will sometimes deviate from their previous course on the sands, and cause the main channels to shift. From the great width of the bay these noble waters cannot, however, materially affect the rate or height of the tide; in fact, on a calm day they may be seen from the hills along the coast†, flowing apparently in their usual direction over the denser sea water, from which they are distinguishable by their superior smoothness of surface.

Off Heysham, Poulton, Hest-bank, Warton, &c., the tides

* Communicated by the Author. † From Warton Crag in particular.

assume in moderate weather the character of *calm* tides*, the slight waves fronting the bottom of the bay with a crest parallel to a line drawn across its entrance, or from south-west to north-east. It is, however, probable that rapid currents are formed by the tide on rounding the headlands which oppose its progress in the lower part of the bay, as I have observed the tide make the entire circuit of Silverdale Bay and Point, strongly agitated and at a tremendous rate, under an opposing wind of great force †.

The time of high water is not the same throughout the bay, the greatest differences being supposed to exist between opposite parts of the shores. Winds from the south-west quarter, blowing *strongly* either in the bay or up the Irish Channel, are considered to accelerate the time of high water and increase the height of the tides ‡, retarding, on the other hand, the time of ebb, and sustaining the waters above their usual level. Some of the highest tides in the bay have taken place under similar circumstances at neaps. To winds from the north-east quarter effects exactly opposite are attributed, those from the south-east or north-west being termed neutral. The highest tides, about thirty feet, are held to be about Peel Castle, where there is only half a fathom of water at spring-tides, low water; but those at Heysham, ten miles east of that locality, it will be proved, are scarcely inferior.

On the Tides at Hest Breakwater.

The breakwater, situate half a mile north-west of Hest-bank, (a village three miles north of Lancaster,) projects about fifty yards in a north-west direction from a gravel bank thrown up as a road to it from the shore, from which it is nearly three hundred yards distant. It is constructed of solid masonry, perpendicular on every side to the height of its surface, which is about level with high-water neap tides. The force of the waves in stormy tides is broken on its south-west side by a connected sloping bank formed of fragments of rock. A lofty pole, formerly used for hoisting a lamp, is fixed perpendicularly within a stone pedestal let into the surface of the breakwater at a distance of seven yards from its north-west

* Between Kent's Bank and Silverdale the tide *is said* to form a *bore*, advancing breast high, with a roaring noise, at the full speed of a horse. (The *bore* of the Ganges runs at the rate of seventeen miles an hour for seventy miles.)

† On the 1st of September last, memorable for the number of shipwrecks.

‡ The sailors are of opinion that strong dews and heavy rains increase the height of a tide (?).

end. The Keer, which, flowing in its usual channel, passed close by the breakwater at the time of its erection (only a few years ago), soon after receded from it considerably to the north-west; and again altering its course after the rainy autumns of 1829 and 1830*, fell into the Grange channel four or five miles above their previous confluence off Poulton. The height of the north-east or lee side of the breakwater, originally upwards of sixteen feet, is now reduced by the gradual deposition of warp to nine feet†. On this account a costly structure for the convenient unshipping of goods destined to be forwarded by the Lancaster canal, is now become unserviceable and suffered to go to ruin‡.

In order to find the time, &c., of high water, the east side of the breakwater pole was divided, from its base upwards, to a sufficient height, into feet, and after a few days' observations into half-feet. A board, similarly divided, was nailed perpendicularly to one of the piles (the nearest to the pole) on the lee side of the breakwater, its upper division being placed exactly level with the base of the pole by means of a spirit-level. From the shore (little above the level of the breakwater) these divisions, which were marked and numbered with white paint, were distinctly seen through a 20-inch telescope, and little difficulty occurred in determining in calm weather, and especially at high tides, the nearest minute by the watch when the water appeared level with any division§. With a rough sea dashing over the breakwater it required most vigilant observation to decide when the waves fluctuated as much above as below the mark, or subsided for some moments to the level of it. The observations at neaps, generally more uncertain, were facilitated by the comparative tranquillity of the water by the divided board. The divisions on the pole and board served also to mark the extreme height of the tide above or below the surface of the breakwater.

When the perpendicular rise and fall of a tide are uniform in rate, half the sum of the times at which it has risen and

* At the same period a low sand-bank, called Priest Skear, lying a mile north of the breakwater, gradually assumed the form of an island. Not many years ago it was pasture land connected with the shore, now half a mile distant.

† This has been explained in Treatises on Harbours. Beneath the warp lies the red marl with ironstone, ranging between Carnforth and Bare. It appears to be the same rock which runs in patches between Ingleton and Kirkby Lonsdale.

‡ About thirty years ago the Ulverston canal was rendered nearly useless by a similar shifting of a branch of the Furness channel.

§ In general it was most difficult to mark with certainty the time when the sea became level with the *base* of the pole.

afterwards descended to the same (height or) division, will give, alike from observations of every division, the time of high water; but as the rate will generally vary, not only from astronomical causes, but also from sudden changes in the force or direction of the wind, it follows that the series of times obtained from the several divisions will neither coincide with each other, nor with the true instant, the error increasing (irregularly) from the highest to the lowest division noted. In the course of observations, frequently protracted to three hours, as the extreme difference between the times obtained never exceeded five minutes, the one derived from the highest division rarely required a correction of moment to reduce it to the true instant of high water.

The watch made use of, which has a detached lever, but is without compensation-curb, &c., keeps a tolerably uniform rate when not exposed, as was sometimes the case, to violent shocks on travelling. Its error was ascertained from time to time at a station near Hest-bank from sets of observations made in the afternoon, of the instants the sun's upper and lower limbs had descended to a certain altitude (measured by a box sextant,) above the sea at the horizon, or later in the afternoon above its margin (?) about Peel Castle, a distance of 15+ miles. The dip of the sea, found by the sector to vary, according to the state of the tide and refraction, from 8′ 0″ to 11′ 28″, was taken at 10′ 0″. On one occasion the sun's height was obtained in the morning by reflection from the tranquil surface of the canal. (See Table I.) From the highest* part of the north-east battlement of the canal bridge on the Oversands road from Hest-bank, go two and a half feet towards the opposite side of the bridge, and the *station*, 51 feet distant, (the east end of the roadside wall,) will be in a line with the breakwater pole. The height of the wall top above the long level of the Lancaster canal (extending from Preston nearly to Burton-in-Kendal,) measured 13 feet. The (trigonometrical) latitude was found to be 54° 5′ 34″ north, and the longitude 2° 48′ 5″ (or 11 min. 12 sec. in time) west of Greenwich. The distance from Hest-bank wall to the Breakwater pole, 2339·3 feet†, was calculated from the angles taken by the four-inch theodolite at both ends of a base line extending 855 feet (as measured by a corrected tape) along the west margin of the Oversands road near the bathing-

* In 1829, the fall to the canal was 17 feet 3 inches, and 17 feet in 1832, the fall to the wall top being 4 feet 1 inch.

† Differing little from, but preferable to, the length obtained by a triangulation connected with that of Colonel Mudge.

house. With this distance, and the vertical angles measured at the wall by the sector, we get

	Feet.
Height of wall above breakwater pole top, (the mean of three observations in 1829 and 1832, varying from 43·1 to 43·2)........................	43·2
Height of wall above pole base, (the mean of eight observations, in 1829, 1830, and 1832, varying from 70·9 to 71·4)........................	71·2

Hence the canal will be 30·2 feet above the top, and 58·2 above the base of the pole.

Notwithstanding the present dilapidated state of the breakwater, the measurements do not indicate any alteration of level in the pedestal of the pole.

Height of Neap and Spring Tides at Heysham.

The tide deserts Hest sands soon after half-ebb, and at Poulton, two miles to the west-south-west, it either ebbs out at spring tides, or is cut off by a two-foot bar of sand. In Heysham lake, 5 miles south-west of Hest-bank, there is, however, constantly deep water, communicating without current (?) with the open sea.

July 20, 1832, two days before the neap tides were at a *minimum*, a stake was driven just within the margin of the lake down to the exact level of its unruffled surface when at the lowest (or about $10^h\ 2^m$ A.M., mean time). The height of high water was marked (in a dead calm) true to an inch or two (at $3^h\ 57^m$ P.M.) by a similar stake driven (820 yards to the eastward of the other) into the sands near the road from Upper Heysham to the shore. The difference of level of the two marks was found by the six-inch theodolite and two six-feet levelling staves, divided on both sides into inches, of which the fractional parts, as the staves were unfurnished with sliding vanes carrying verniers, were visually estimated. The level (bubble) of the theodolite was properly adjusted, but from a defect in the screw-key, the line of collimation, which pointed too high by one tenth of an inch in a distance of fifty feet, could not be rectified. To meet the evil, the instrument was invariably placed (between and) equidistant from the two staves previously set up perpendicularly (by a plumb-line) at distances from each other not exceeding 100 feet*. Two staves were indispensable on account of the slight, yet gradual

* Or, when the *sum* of the distances from the instrument to the staff in one direction equal the *sum* of those to the staff in the other direction, a compensation of errors takes place.

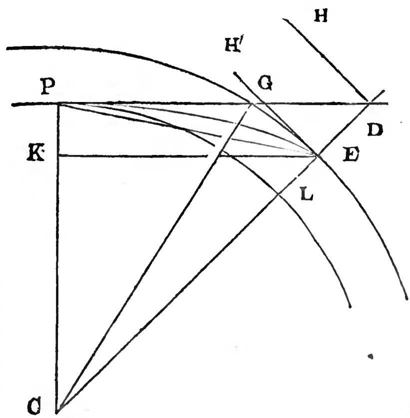
sinking of the heavy instrument within the sand, which required that both levels should be taken as nearly at the same time as possible*. The total fall appeared to be 18 feet 6 inches, but a solitary limestone, considered as the standard height for extreme neaps, was situated 1 foot 9 inches below the level of our mark, which would reduce the total height of the tide by double (?) that quantity, or to 15 feet.

July 28, 1832, the day previous to the highest spring tide (the weather being perfectly calm), the fall from the level of high water (at $11^h\ 32^m$ A.M.) to our stake near the shore measured 6 feet 5 inches, but that from the stake by the lake down to the level of the latter at low water (at $5^h\ 57^m$ P.M.) did not exceed 4 feet 7 inches, although a continuous fall was perceptible nearly up to the time quoted†. The total height of the tide will therefore be 29 feet 3 inches. On this occasion, as the theodolite was completely adjusted, the difference of level between the two stakes driven down at neaps was carefully remeasured, and made to be 18 feet 3 inches, or only 3 inches less than previously.

In strictness the rise as well as the fall of a tide should be obtained, and the *mean* taken for its correct height.

Measurement by the Dip of the Height of the Tides.

No ray of light originating at any point (P) on the (spherical) surface of the sea (PL) could pass below the level of the horizontal plane PD (which is perpendicular to the vertical PC) without being absorbed in the sea. At the point of intersection (D) of this plane with any other vertical, as DC, the angle HDP, or depression of P below the horizontal plane or line DH, is termed the true dip, and is equal to the arc contained between the two verticals PC and DC, or the angle PCD.



* As an additional precaution one staff was read off *before* as well as *after* noting the other, and the mean of both readings registered.

† The fishermen hold that there is a difference of half an hour between the duration of ebb and flow in Heysham lake.

The ray from P, initially in the direction of D, being constantly curved downwards in its passage from the effects of atmospherical refraction, will cut the vertical DC at a point E lower than D by (nearly) the tangent of an angle DPE, which bears a constant proportion to the true dip (or contained arc). At E the depression of P is evidently less than at D by the angle PEK = DPE, or the angle of refraction; but as P will not be seen from E in the direction EP, but in a tangent to the extremity of the curved ray at P, it will appear elevated above P by the angle of refraction: hence the *apparent* dip at E will be less than the true dip at D by twice the angle of refraction*.

DL being the height of D above the level of the sea, that of E will be equal to DL *minus* DE (or the part cut off by refraction). As the angle DPL is constantly half the dip at D, (or half the contained arc PCD,) it follows that the angle DPE will bear twice the proportion to that of DPL that the refraction does to the contained arc. When the height in feet of E is given, that of D may be found by increasing E by $\frac{2\,E}{n-2}$, n being the ratio of the contained arc to the refraction. Thus, if E be 80 feet, and the refraction $\frac{1}{10}$th of the arc, then will the height of D be $80 + \frac{80 \times 2}{10-2} = 100$ feet. The true dip for D being 10′ 37″, that for E will be (10′ 37″ minus $\frac{1}{10}$th, or) 9′ 33″, and its apparent dip (10′ 37″ minus $\frac{1}{5}$th, or) 8′ 30″. (The true dip for G (= 80 feet) would be 9′ 30″, or 3″ less than for E.)

Although well aware that the difference between the true and apparent dip was subject to great and unaccountable fluctuations, yet I flattered myself that by measuring along with the depression of the horizon of the sea that of a fixed object on the shore beyond, situate nearly in the same direction and at about an equal distance, I should then be able to ascertain the deviation of the refraction at low water from its amount at high water, the latter being equal to the difference between the apparent dip and that calculated from the measured height of the eye above the sea at the breakwater†. The plan, which, with one exception, appears to have been

* G, the point of intersection of the unrefracted ray PD with a vertical at a height equal to that of E, has a less extensive horizon than the latter by (twice) the arc GE. D is elevated above E, yet their horizons, as that of the former is not affected by refraction, are equal.

† Any probable difference in the times of high water at the horizon of the sea and at the breakwater would scarcely exceed the duration

tolerably successful, would probably have been completely so had the *base* of Walney lighthouse (the object selected for reference) been observed in lieu of its ill-defined conical *top*. At Hest-bank *wall*, where the measurements were made by the sector, the height, though scarcely more than requisite to obtain at low water an unobstructed view of the open sea, throws its horizon to a distance of 9 to 11 miles, and thus renders it necessary to have the dip true to 3″ or 4″ in order to insure to the calculated height of the eye the accuracy of one foot. The bank is steep, and extends to the south-west and north-east. At high water the station may be 200 yards distant from the sea in the direction observed, (a little left of the lighthouse, which lies to the west by south,) but at low water sands, intersected by channels and pools of water, intervene for several miles.

Generally each day's observations are arranged below, in an order to exhibit,—1st, The variation at low water of the depression of the lighthouse from its amount at high water; 2nd, the refraction at high water, or difference between the observed dip and that calculated from the measured height of the eye; 3rd, the height of the eye at low water, computed from the observed dip corrected by a refraction differing from that obtained at high water by the corresponding variation in the depression of the lighthouse; 4th, the dip answering to the height of the eye at low water, derived from a table, founded on the measurements at Heysham; 5th, the consequent error of the estimated refraction and height of the eye.

September 14th, 1829.

(At 0^h 5^m P.M. Black Comb, 126,325 feet distant } 42′ 21″ elev.; refr. $\frac{1}{31}$)

10 17	A.M. Lighthouse top 79,863 feet distant	5 43 depr.	}	5′ 46″
0 5	P.M.	5 49 —		
4 20	....	5 $34\frac{1}{2}$ —	}	5 33
6 10	....	5 $31\frac{1}{3}$ —		
		Variation		0 13

of the *hang* of the tide. Granting even a difference of level in the sea at high water at the two localities, we should then form a false estimate of the refraction, yet as the calculated dip at low water would be nearly equally in defect or excess with that at high water, their difference, or height of the tide, would not be materially affected.

At 10h 2m A.M.	Dip	8′ 53″;	Eye	70·2 feet;	refr.	+0′ 1″
10 35	——	8 30	—	67·5	——	0 13
11 0	——	8 24½	—	65·4	——	0 11
11 30	——	8 20½	—	64·2	——	0 10
11 46	——	8 24½	—	64·4	——	0 6½
11 58	——	8 22	—	64·7	——	0 10
0 30 P.M.	——	8 30	—	66·7	——	0 10
Mean ……		8 29				+0 8½
Half variation	…	…	…	…	…	+0 6½

Mean refraction $\left(\frac{0'\ 15''}{8\ 37\frac{1}{2}}, \text{ or}\right)$ $\frac{1}{34}$th. … … +0 15

At 5h 12m P.M.	Dip	9′ 58″	Low water.
5 30	——	10 0	
5 45	——	9 58	
6 0	——	9 55	

Mean … … … 9 58
Mean refraction $\frac{1}{34}$ +0 18½
Half variation … +0 6½

Corrected dip and height of eye	10′ 23″	=	95 6 feet.
Tabulated do. … …	10 27	=	97·0
Errors … … …	−0 4		−1·4

September 15th 1829.

At 10h 50m	A.M.	Lighthouse top	5′ 49″ depr.	5′ 50″
0 55	P.M.	——	5 51 —	
6 0	A.M.	——	4 35 —	
(6 5	A.M.	Black Comb	42 48 elev.; refr. $\frac{1}{19}$.)	
5 45	P.M.	Lighthouse top	5 40½ depr.	

Morning variation −1′ 15″; evening variation −9½″.

At 10h 43m A.M.	Dip	8′ 45½″;	Eye	71·3 feet;	refr.	+0′ 12″.
11 5	——	8 49	—	68·7	——	−0 1
11 47	——	8 30½	—	64·9	——	+0 3
0 20 P.M.	——	8 29	—	64·0	——	+0 0½
0 48	——	8 23	—	64·5	——	+0 8½
Mean ……		8 35½				+0 4½

Mean *morning* refraction $(4\frac{1}{2} + 37\frac{1}{2}) = \frac{0'\ 42''}{8\ \ 40} = \frac{1}{12\cdot 3}$.

Do. *evening* do. $(4\frac{1}{2} + 5\ ) = \frac{0\ \ 9\frac{1}{2}}{8\ \ 40} = \frac{1}{53}$.

At $5^h 45^m$ A.M.; Dip ...	8′ 43″	Low water, morning.	
6 15 ——— ...	8 41		
Mean	8 42		
Refraction $\frac{1}{12\cdot3}$ ...	+ 48		
Half morning variation	+ $37\frac{1}{2}$		
Corrected dip and height of eye	10 $7\frac{1}{2}$	= 91·0 feet.	
Tabulated do.	10 $30\frac{1}{2}$	= 98·0	
Errors	−0 23	−7·0	

At $5^h 31^m$ P.M. Dip	9′ 50″	Low water, evening.	
6 0 ———	9 50		
6 20 ———	9 52		
Mean	9 51		
Refraction $\frac{1}{33}$	+ $11\frac{1}{2}$		
Half evening variation	+ 5		
Corrected dip and height of eye	10 $7\frac{1}{2}$	= 91·0 feet.	
Tabulated do.	10 $30\frac{1}{2}$	= 98·0	
Errors	− 0 23	−7·0	

In the morning there was a strong white frost, which did not recur in the evening. To account for our estimate of the refraction being so much in defect, we must either suppose our tabulated height of the (equinoctial) tide (34 feet) in excess, or that the variation of refraction was less for the lighthouse than the horizon of the sea, and to precisely the *same* amount in the morning as evening! At noon, which was bright and warm, the refraction for the dip and the lighthouse were both less by $4\frac{1}{2}''$ than on the preceding day.

September 16th, 1829.

(At $2^h 35^m$ P.M.	Black Comb,	42′ $33\frac{1}{2}''$	elev.; refr.	$(\frac{1}{24})$
(6 55 A.M.	do.	42 $55\frac{1}{2}$	———	$\frac{1}{17}$
0 35 P.M.	Lighthouse top	5 $33\frac{1}{2}$	depr.	
6 45 A.M.	do.	5 12	—	
(10 25 A.M.	do.	5 17	—)	

Variation $-21\frac{1}{2}''$

At $0^h 23^m$ P.M.	dip 8′ 17″;	Eye 66·3 feet;	refr.	+ 21″
0 41 ———	8 10	— 65·6	——	$25\frac{1}{2}$
1 0 ———	8 4	— 65·3	——	31
1 18 ———	8 4	— 65·2	——	31
1 40 ———	8 1	— 65·6	——	$34\frac{1}{2}$
Mean ...	8 7			+ $28\frac{1}{2}$
Half variation				+ $10\frac{1}{2}$
Mean refraction ...	$\left(\frac{0' \ 39''}{8 \ 35\frac{1}{2}}\right) = \frac{1}{13}$.			+ 39

At 6h 30m A.M. dip	... ...	11′ 13″	Low water.	
7 0 ———	... ...	11 28		
Mean	... ...	11 20½		
Refraction $\frac{1}{13}$	... ...	+57		
Half variation	... ...	+10½		
Corrected dip and height of eye		12 28	= 138·0 feet.	
Tabulated do.		10 25	= 96·3 —	
Errors	... ...	+2 3	+41·7 —	

The refraction for the dip at noon is unusually great, but that for Black Comb and the lighthouse, is nearly equally in excess. The morning was frosty, and the horizon of the sea, although dull at the first observation for low water, became quite clear at the second. The refraction appears, however, to have been 55½″ *negative*; a probable consequence of a frosty morning succeeding a warm evening, in which case the air would be considerably cooler than the sea. Under similar circumstances, Biot has observed the dip *at sea* to be 4 or 5 minutes greater than usual.

August 24th 1829.

At 2h 0m P.M. (Low water) dip	10′ 0½″	
2 30 ———	9 52½	
Mean	9 56½	
Mean *noon* refr. Sept. 14, 15, 16 = $\frac{1}{38}$	+15½	
Corrected dip and height of eye	10 12	= 92·3 feet.
Tabulated do. ...	10 4½	= 90·0 —
Errors	+0 7½	+2·3 —

Height of eye at high water 92·6 feet. Lighthouse unobserved.

August 25th, 1829.

At 2h 30m P.M. Low water, dip	9′ 38″	
2 45 ———	9 38	
3 15 ———	9 41	
Mean	9 39	
Mean noon refraction $\frac{1}{38}$	+15	
Corrected dip and height of eye	9 54	= 87·0 ft.
Tabulated do.	10 5½	= 90·3
Errors	−0 11½	−3·3

Height of eye at high water, 72 feet. Lighthouse unobserved.

October 13th, 1830.

At $9^h\,22^m$ A.M. Lighthouse top, depr. $5'\,40\frac{1}{2}''$.

High water.
At 9 32 A.M. dip $8'\,33''$; eye 71·3 ft.; refr. $+24\frac{1}{2} = \frac{1}{22}$.

Between the lighthouse and horizon of the sea, there appeared a bank of white vapour.

October 14th, 1830.

High Water.

At $9^h\,42^m$ A.M.	dip $8'\,41\frac{1}{2}''$;	eye 70·7 ft.;	refr.	$+15\frac{1}{2}''$
10 12	8 $39\frac{1}{2}$	70·1	——	$13\frac{1}{2}$
10 42	8 $40\frac{1}{2}$	70·7	——	$15\frac{1}{2}$
Mean ...	8 $40\frac{1}{2}$			$+15 = \frac{1}{31\cdot6}$.

At 4 35 P.M. Low water; dip	$9'\,35''$	
Refraction at noon $\frac{1}{36}$ ...	$+16\frac{1}{2}$	
Corrected dip and height of eye	9 $51\frac{1}{2}$	= 86·2 ft.
Tabulated do.	10 11	= 92·0
Errors	$-0\ 19\frac{1}{2}$	$-5\cdot8$

The refraction $(+36'')$ must have been more than double its value at the time of high water.

October 15th, 1830.

High water.

At $9^h\,58^m$ A.M.	dip $8'\,45\frac{1}{2}''$;	eye 70·7 ft.;	refr.	$+10\frac{1}{2}$
10 19	8 $37\frac{1}{2}$	69·7	——	$14\frac{1}{2}$
10 48	8 40	68·3	——	$6\frac{1}{2}$
11 16	8 $39\frac{1}{2}$	69·7	——	$12\frac{1}{2}$
Mean ...	8 $40\frac{1}{2}$	...		$+11 = \frac{1}{48}$.

October 16th, 1830.

High-water.

At $11^h\,8^m$ A.M.	dip $8'\,41''$;	eye 68·5 ft.;	refr.	$+5$
11 31	8 35	68·4	——	12
Mean ...	8 38			$+8\frac{1}{2}$ or $\frac{1}{62}$.

Table I. *Observations for determining the Error of the Watch.*

1832.	Altitude.	Sun's L.L.	Sun's U. L.	Error, mean time.	Daily Rate.
July 7. P.M.	29° 30′	4h 49m 25s	4h 53m 0s	+ 3m 30s	sec.
	28 30	4 56 5	4 59 5		+38
10.	32 5	4 32 0	4 35 30	+ 5 22	
12.	31 0	4 39 0	4 42 50	+ 6 30	+34
	30 0	4 46 35	4 49 45		
14.	32 0	4 31 40	4 35 25		+25
	30 30	4 42 14	4 45 55	+ 7 20	
	29 0	4 52 40	4 56 15		+60
18.	24 0	5 27 10	5 31 0	+11 23	
	23 0	5 34 30	5 38 20		
23.	32 30	4 28 25	4 32 20		+49
	31 30	4 35 5	4 39 15	+15 26	
	30 30	5 42 10	* * *		
26.	22 30	5 35 10	5 38 52		+33
	21 30	5 42 0	5 45 47	+17 5	
	20 30	5 48 50	5 22 20		+20
†Aug. 1. A.M.	19 45	7 2 25	* * *	+18 56	
7. P.M.	21 0	5 34 45	5 38 20	+23 28	+42

Mean rate from first to last = +39 seconds.

Table II. *Register of the Tides at Hest-Bank.*

Explanation of the Columns.—1st, The day of the month. 2nd, The *number* of divisions (on the breakwater pole) observed: the asterisk denotes that the corresponding times for high water increased or decreased *seriatim*. 3rd, The height of the top of the pole above the level of the tide at high water. 4th, The instant of high water calculated immediately from the times, by the watch, at which the tide reached (during the flow and ebb) the level of the several divisions observed. 5th, The last column corrected for the error of the watch, mean time, Hest-bank. 6th, The predicted time (by Holden's Tables) of high water at Liverpool, reduced to mean time and the meridian of Hest-bank‡. 7th, The direction and force of the wind. 8th, The state of the surface of the sea.

† By Reflection, (see page 267 for Dip, &c.)

‡ The latitude of Liverpool is 53° 24′ 40″ north, and its longitude 2° 58′ 55″ west.

					Hest-bank.	Liver-pool.	Wind.	Sea.
1832.	Div.	Ft.	In.					
July 7.	9*	28	3†	$6^h 48^m 0^s$ A.M.	$6^h 45^m$	$6^h 43^m$	E.; very little.	Ripple; smooth.
	4	28	9	7 27 0 P.M.	7 23	7 19	S.W. ?...........	Rough; calmer.
9.	3	27	9	8 51 0 A.M.	8 46	8 54	S.W.; strong.	Waves great.
10.	3	27	4	9 44 0 A.M.	9 39	9 43	W.S.W.; gentle.	Rather rough.
11.	4*	26	3	10 30 0 A.M.	10 24	10 26	W.; strong; W.S.W. stronger.	Do.
12.	3*	26	3	11 6 30 A.M.	11 0	11 3	W.S.W; W. gentle.	Do.
13.	4*	25	9	11 42 30 A.M.	11 36	11 36	N. by E.; little.	Nearly smooth.
14	5	25	10	0 19 0 P.M.	0 12	0 11	N.; W.N.W.; W.; rather strong....	Calm; rather rough.
16.	4*	25	6	1 27 0 P.M.	1 18	1 18	W.S.W.; W.; blustry	Very rough.
17.	3	26	3	2 2 30 P.M.	1 52	1 51	W. by S.; blustry.	Do.
18.	2	27	0	2 42 0 P.M.	2 31	2 29	W.N.W.; strong and steady	Waves great.
19.	2	28	0	3 24 0 P.M.	3 12	3 9	W.; gentle	Slight swell.
21.	2	29	7	5 12 30 P.M.	4 59	4 54	N.N.E.; gentle.	Nearly smooth.
23.	4*	28	4	6 56 0 A.M.	6 41	6 39	W. by N.; gentle.	Do.
	3*	28	6	7 37 30 A.M.	7 22	7 15	N.W.; W.N.W.; rather strong.	Rather rough.
24.	5*	27	10	8 5 0 A.M.	7 49	7 49	W.; calm.........	Smooth.
25.	7*	26	4	9 8 30 A.M.	8 52	8 55	N.W.; calm.....	Smooth; ripple.
26.	7*	24	9	10 7 0 A.M.	9 50	9 53	S.E.; S.W.; gentle	Slight ripple.
27.	9*	23	6	11 2 15 A.M.	10 45	10 45	N.E.; W.; calm.	Nearly smooth.
30.	7*	22	6‡	1 28 0 P.M.	1 10	1 10	W.; slight.......	Smooth.
31.	7*	23	5	2 17 0 P.M.	1 58	1 57	N.; gentle.	Do.
Aug. 1.	2	25	0	3 7 0 P.M.	2 48	2 41	E. by S.; rather strong.	Do.
2.	3*	26	5	3 54 45 P.M.	3 35	3 28	E. by N.; gentle.	Rather rough.
4.	3	29	4	5 52 0 P.M.	5 31	5 17	W.; calm.	Smooth.
6.	1	30	0	7 39 0 A.M.	7 17	7 6	W.; calm.	Do.
7.	1	29	9	8 49 45 A.M.	8 27	8 19	W.; slight	Gentle ripple.
1829.								
Sept. 14.	...	20	7	11 30 0 A.M.	...	...	N.N.W.	Smooth.
15.	...	20	2	0 15 0 P.M.	...	...	W.; brisk.	Ripple.
16.	...	21	5	1 10 0 P.M.	...	...	N.N.E.; calm.	Smooth.
1830.								
Oct. 13.	...	27	8	9 35 0 A.M.	...	...	S.E.; calm.......	Do.
14.	...	26	6	10 10 0 A.M.	...	...	S.E.; slight	Do.
15.	...	25	3	10 45 0 A.M.	...	...	S.E.; calm.......	Do.
16.	...	24	8	11 21 0 A.M.	...	...	?; calm..........	Do.
19.	...	24	0	0 49 0 P.M.	...	...	S.S.E.; slight...	Rather rough.
20.	...	23	6	1 16 0 P.M.	...	...	S.; very strong.	Very rough.

† These numbers *plus* 30·2 feet will give the height of the Lancaster canal above the tides at high water.

‡ The preceding tide would be about 21 feet 6 inches below the top of the pole.

N. B. The moon's phases were,

New.	June 28th, at	$6^h 50^m$	A.M.	Hest-bank, mean time.
Full.	July 12th,	10 49	P.M.	
New.	July 27th,	1 50	P.M.	
Full.	Aug. 11th,	2 21	P.M.	

Chapel Allerton, near Leeds,
July 24, 1834.

JOHN NIXON.

XXXIX. *On the Influence of high and low Prices on the Rate of Mortality.* By JOHN BARTON, *Esq.**

IN a pamphlet which I published rather more than twelve months ago †, I stated at considerable length the results of an investigation into the influence of high and low prices on the condition of the people, as evidenced by the rate of mortality in the kingdom at large, as well as in our agricultural and manufacturing districts separately. These results were deduced from a comparison of the number of burials in the population returns with the price of wheat in each year, from 1780, the commencement of these returns, down to 1820, the returns of the succeeding years not being at that time printed. The subsequent completion of the work enables me now to state the results of fifty years' experience in this matter. At the same time I shall be glad to take the opportunity of saying a few words on an objection which has been urged against my conclusions by a writer in the Edinburgh Review.

The general result of my inquiry respecting the influence of the price of corn on the rate of mortality in the kingdom at large will be seen in the following statement:

Price of Wheat per Winchester Quarter reduced to Bullion Value.	Average Burials yearly on each Million of Population.
Under 50*s*.	22·455
50*s*. to 60	20·175
60 to 70	19·778
70 to 80	19·291
80 to 90	18·257
90 to 100	18·117
above 100	22·350

* Communicated by the Author.

† Entitled, "An Inquiry into the Expediency of the existing Restrictions on the Importation of Foreign Corn."

From a comparison of these numbers, I inferred,

"1st, That the extremes of high and low prices are both unfavourable to the comfort and health of the labouring classes.

"2nd, That of the intermediate prices, those are most favourable which approach the higher extreme."

The objection taken by the reviewer to this reasoning, I will state in his own words.

"Mr. Barton, in the pamphlet placed at the head of this article, endeavours to prove that a high price of corn diminishes mortality; and in favour of this singular opinion he produces tables of burials, prices, &c., from 1780. But, whatever this gentleman may think, it does not necessarily follow, that because two circumstances are consentaneous, the one is therefore the cause of the other. The improved condition and habits of the labouring and middle classes since 1780, coupled with the extirpation of the small-pox, and other improvements in medical science, have occasioned a diminution in the rate of mortality, which has more than balanced the contrary influence of the increased price of corn. Mr. Barton would have been quite as logical, and quite as correct, had he ascribed the diminished mortality to the increase in the number of steam-engines, or to the more general employment of children in factories."—No. 118. p. 306.

Does the reviewer mean, I would ask, to assert, that the price of corn has been progressively and uniformly rising since 1780, and the rate of mortality progressively and uniformly decreasing during the same period? If not, I do not see the force of his reasoning. If he does, a simple statement of figures may show how widely he is mistaken.

Periods.	Average Bullion Price of Wheat.	Rate of Mortality*.
1801—1810	*76s. 9d.*	1 in 46·4
1811—1820	*75s. 1d.*	1 in 52·7
1821—1830	*57s. 6d.*	1 in 51.

It is true, as the reviewer observes, that the mere consentaneous occurrence of two events does not afford any proof, scarcely a presumption, that any connexion exists between them. If, on throwing a die, it falls with the face marked *six* uppermost, I have no reason to suppose it loaded. But if, on throwing it many times in succession, the same face invariably presents itself, a very strong presumption arises that the die is operated upon by some secret bias. In like manner, if the irregular fluctuations in the price of corn are found so to fit

* Population Returns of 1831, vol. iii. p. 489.

and dovetail into the irregular fluctuations of the rate of mortality, that the results, when collected and arranged, exhibit all the uniformity of a permanent law of nature, will any person of competent judgement assert that the coincidence is accidental?

As far as the objections of the reviewer are concerned, I might, I think, content myself with these observations; but as it is a matter of considerable importance and curiosity, not merely to determine in general terms whether low prices exercise an unfavourable influence on human life, but to obtain a measure of the *quantum* of that unfavourable influence, I have sought for the best means of eliminating the circumstances alluded to by the reviewer, so as to present a correct statement of the influence of the price of corn on the rate of mortality, freed from all disturbing causes. The most likely method of accomplishing this purpose seems to be to select for the basis of our calculation a period such, that on dividing it into two equal portions, the average price during each of these portions shall be as nearly equal as circumstances admit. Thus, if we select for this purpose the forty years from 1791 to 1830, we shall have

The average bullion price of wheat from	1791 to 1810	= 69*s.* 0*d.*
The same from	1811 to 1830	= 66*s.* 3*d.**

If now we suppose a progressive and uniform improvement in medical science and in the habits of the people to have taken place during these forty years, it is evident that a nearly equal proportion of the influence of these favourable circumstances will fall on years of high price and on years of low price, since these years are so equally distributed through the two periods as to give for the average of the first twenty years, within a trifle, the same sum as for the average of the last twenty years.

The following Table contains the results of that calculation:

* If we exclude the years of scarcity, 1795, 1800, and 1801, we have

The average of the first nineteen years	65*s.* 0*d.*
The same of the last eighteen years	65*s.* 9*d.*

Years.	Female Population.	Burials of Females.	Burials on each Million of Population when the Price of Wheat is							Wheat per Quarter.	
			under 50s.	50s. to 60s.	60s. to 70s.	70s. to 80s.	80s. to 90s.	90s. to 100s.	above 100s.	In Paper.	In Bullion.
1791	4,140,000	92,668	22,380	...	...	...	...	...	...	*s. d.*	47s 0d
1792	4,190,000	94,794	22,620	...	...	...	...	...	...	...	42 11
1793	4,245,000	101,699	23,960	...	...	...	...	...	...	...	48 11
1794	4,292,000	98,933	...	23,050	...	...	...	...	...	...	51 8
1795	4,336,000	104,747	...	...	...	24,160	...	...	...	...	74 2
1796	4,374,000	95,426	...	...	...	21,820	...	...	...	...	77 1
1797	4,421,000	95,825	...	21,670	...	...	...	...	...	...	53 1
1798	4,475,000	93,765	20,950	...	...	...	...	...	...	50 3	48 10
1799	4,532,000	94,348	...	...	20,820	...	...	...	...	67 6	64 7
1800	4,587,000	102,909	...	...	...	...	...	...	22,430	113 7	104 0
1801	4,628,000	103,082	...	...	...	...	...	...	22,270	118 3	108 3
1802	4,659,000	100,385	...	...	21,550	...	...	...	...	67 5	63 2
1803	4,713,000	101,269	...	21,490	...	...	...	...	...	56 6	55 0
1804	4,777,000	89,639	...	18,760	...	...	...	...	...	60 1	58 5
1805	4,853,000	90,154	...	...	...	...	18,580	...	...	87 10	85 5
1806	4,928,000	91,163	...	...	...	18,500	...	...	...	79 0	74 6
1807	5,003,000	97,855	...	...	19,560	...	...	...	...	73 3	69 1
1808	5,074,000	98,149	...	...	...	19,340	...	...	...	79 0	74 6
1809	5,142,000	93,577	...	...	...	...	18,200	...	...	95 7	81 9
1810	5,218,000	103,277	...	...	...	...	...	19,790	...	106 2	97 2
1811	5,283,000	93,572	...	...	...	17,710	...	...	...	94 6	76 11
1812	5,364,000	94,445	...	...	...	...	...	17,610	...	125 5	96 7
1813	5,442,000	92,751	...	...	...	17,040	...	...	...	108 9	79 8
1814	5,529,000	102,878	...	18,610	...	...	...	...	...	73 11	59 11
1815	5,608,000	97,966	...	17,470	...	...	...	...	...	64 4	54 5
1816	5,708,000	102,005	...	...	...	17,870	...	...	...	75 10	73 9
1817	5,794,000	98,229	...	...	...	...	...	16,950	...	94 9	92 9
1818	5,886,000	105,900	...	...	...	...	17,990	...	...	84 1	80 3
1819	5,970,000	106,815	...	...	...	17,890	...	...	...	73 0	71 3
1820	6,053,000	104,020	...	...	17,180	...	...	...	...	Imperl	65 7
1821	6,144,000	104,870	...	17,070	...	...	...	...	...	56 2	54 5
1822	6,247,000	109,116	17,460	...	...	...	...	...	...	44 7	43 2
1823	6,355,000	117,737	...	18,520	...	...	...	...	...	53 5	51 9
1824	6,453,000	120,047	...	...	18,600	...	...	..	...	64 0	62 0
1825	6,550,000	125,291	...	...	19,130	...	...	...	...	68 7	66 6
1826	6,644,000	132,061	...	19,880	...	...	...	...	...	58 9	57 0
1827	6,734,000	122,880	...	18,250	...	...	...	...	...	56 9	55 0
1828	6,829,000	125,318	...	18,350	...	...	...	...	...	60 5	58 7
1829	6,933,000	129,705	...	...	18,710	...	...	...	...	66 3	64 2
1830	7,026,000	124,777	...	...	17,760	...	...	...	...	64 3	62 3
Average of forty years			21,474	19,375	19,163	19,292	18,257	18,116	22,350		

Few persons, I believe, on comparing the numbers at the foot of this Table with the price of corn, will dispute that it furnishes strong evidence of the tendency of low prices to abridge human life. But I confess I was startled, on taking

the averages, with the anomaly exhibited by the fourth column, which exhibits the rate of mortality in those years when the price of wheat was between 70*s*. and 80*s*. per quarter. It will be seen that the number at the foot of this column, instead of being less than the preceding number, agreeably to the general rule, is somewhat greater. So invariable were the results that I had obtained in my former calculations,—so constantly had I found that every decline of price below 100*s*. per quarter is followed by an increase of deaths, that I was surprised to meet with any exception to this general law. On a little further examination, however, I discovered that this is just one of those cases in which the exception serves to confirm the rule. The anomaly in question depends entirely on the peculiar circumstances of the year 1795, which was, in fact, a year of severe scarcity, as any one may ascertain by referring to Burke's "Thoughts and Details on Scarcity," or other contemporary writings. The mortality of this year ought, therefore, to be classed with the numbers in the last column, though the average price of wheat did not exceed 74*s*. 2*d*.; since it is evident that all scarcity is relative, and that the same price which denotes a moderately abundant crop at one period, may indicate a general failure at another period. Omitting, therefore, the year 1795, the average number of burials per million of population, in those years when the price of wheat fluctuated between 70*s*. and 80*s*., is found to be 18·596,—intermediate between the numbers on either side,—and the regularity of the results is restored.

It may serve further to elucidate this subject, and to confirm the conclusions which I have been endeavouring to establish, if I show that on dividing the fifty years embraced by the Population Returns into five equal periods, the general law above enounced holds good with regard to each of these five periods separately. The middle price, in every case, will be found accompanied by a lower rate of mortality than the extreme either of high or of low price. It is scarcely necessary to observe how powerful must be the influence of the price of corn on the duration of human life, when it can so far overcome all the other circumstances affecting the rate of mortality as invariably to make itself sensibly felt within the short interval of ten years. I am somewhat curious to know whether the reviewer, after looking over these statements, will still continue to maintain his opinion that low prices are favourable to the health and longevity of the people.

Year.	Low Price.	Year.	Middle Price.	Year.	High Price.	Year.	Low Price.	Year.	Middle Price.	Year.	High Price.
	Under46*s*.		46*s*. to 55*s*.		Above 55*s*.		Under70*s*.		70*s*. to 90*s*.		Above90*s*.
1780.	26,650	1782.	24,810			1802.	21,550	1805.	18,580	1800.	22,450
1781.	26,150	1783.	24,780			1803.	21,490	1806.	18,500	1801.	22,270
1786.	23,970	1784.	25,560			1804.	18,760	1808.	19,340		
1787.	23,670	1785.	25,060			1807.	19,570	1809.	18,200		
1788.	23,800	1789.	23,230								
Average	24,848		24,688			Average	20,342		18,655		22,360
	Under 50*s*.		50*s*. to 70*s*.		Above 70*s*.		Under75*s*.		75*s*. to 95*s*.		Above95*s*.
1791.	22,380	1790.	22,940	1795.	24,170	1814.	18,610	1811.	17,710	1810.	19,790
1792.	22,620	1794.	23,040	1796.	21,820	1815.	17,470	1813.	17,040	1814.	17,610
1793.	23,950	1797.	21,690			1816.	17,870	1817.	16,950		
1798.	20,950	1799.	20,820			1819.	17,890	1818.	17,990		
Average	22,475		22,122		22,995	Average	17,960		17,422		18,700

Year.	Low Price.	Year.	Middle Price.	Year.	High Price.
	Under 62*s*.		62*s*. to 70*s*.		Above 70*s*.
1821.	17,070	1820.	17,180		
1822.	17,460	1825.	19,130		
1823.	18,520	1829.	18,710		
1824.	18,600	1830.	17,760		
1826.	19,880				
1827.	18,250				
1828.	18,350				
Average	18,304		18,195		

Recapitulation.

Periods.	Rate of Mortality at Low Price.	Rate of Mortality at Middle Price.	Difference in favour of Middle Price.
1780—1789.	24,848	24,688	160
1790—1799.	22,475	22,122	350
1800—1809.	20,342	18,655	1,687
1810—1819.	17,960	17,422	538
1820—1830.	18,304	18,195	109

Stoughton, May 21, 1834.

XL. *On the Reappearance of* Halley's *Comet.*

IT has generally been considered that this comet will not be seen till towards the end of the ensuing year 1835; and the ephemerides of its place have been computed on this supposition. But Sir Thomas Brisbane has recently received a letter from Mr. Rumker, who says that he thinks it may possibly be seen with a good telescope as early as December in the *present* year; and he has transmitted an ephemeris, computed from the elements of M. Pontécoulant, which, through the kindness of Sir Thomas Brisbane, we are here enabled to present to our readers, and which, perhaps, may be the means of detecting this singular and remarkable body nearly a twelvemonth earlier than was expected.

Ephemeris of the Comet for Mean Noon in Greenwich.

Day of the Month.	Right Ascension.	Declination.	Logar. of the Comet's Distance from the Sun.	Logar. of the Comet's Distance from the Earth.	Passage over the Meridian.
1834.	h m s	° ′			h m
Dec. 16.	5 35 10	+12 18	0·65740	0·55315	11 54·6
26.	5 20 52	12 18	0·64780	0·54382	11 4·7
1835.					
Jan. 5.	5 6 32	12 20	0·63787	0·53924	10 10·7
15.	4 53 0	12 26	0·62760	0·53941	9 17·6
25.	4 40 52	12 36	0·61694	0·54357	8 26·0
Feb. 4.	4 30 40	12 43	0·60589	0·55058	7 36·2
14.	4 22 26	13 7	0·59440	0·55917	6 50·4
22.	4 17 30	13 24	0·58502	0·56681	6 11·8
March 2.	4 13 46	13 43	0·57503	0·57414	5 36·5
10.	4 11 22	14 4	0·56486	0·58115	5 2·4
18.	4 9 26	14 27	0·55432	0·58735	4 28·9
26.	4 9 55	14 51	0·54339	0·59249	3 57·7
April 3.	4 10 40	15 17	0·53203	0·59633	3 26·8

XLI. *On the Internal Structure of Plants.* *By the Rev.* Patrick Keith, *F.L.S.*

[Concluded from p. 188.]

Elementary Organs.

SUCH is our analysis of the composite organs, from which it appears that they are all ultimately reducible either to epidermoid *laminæ*, or to cells, or to longitudinal fibre; which we must consequently regard as being, under one modification or other, the ultimate and elementary organs of the whole fabric of the plant. Are the fibres of plants tubular?

If the stem of a plant of Marigold is divided by means of a transverse section, the divided extremities of the longitudinal fibres, arranged in a circular row immediately within the bark, will be distinctly perceived, and their tubular structure demonstrated, by means of the orifices which they present, particularly when the stem has begun to wither. The tubular structure of the longitudinal fibres of woody plants is not so easily demonstrated. We might infer it, however, from the force and facility with which the sap ascends to the very summit of the stem in the spring and summer, and there are some cases in which we may discern it even with the naked eye. On the horizontal section of a piece of wood that has been long exposed to the action of the atmosphere, the orifices of the longitudinal tubes will appear arranged in circular rows in the direction of the concentric layers. Further, Hedwig affirms that he observed them on the transverse section of a branch of the pear-tree, detached even in the spring, while the sap was yet flowing *. Hence we may believe that the longitudinal fibres of plants are, in fact, longitudinal tubes containing or conveying the alimentary juices. They exhibit a considerable variety of structure, and have been distributed by botanists into several distinct species.

We will take them in the order, and under the anatomical designations introduced or adopted by M. Mirbel, as being one of the most distinguished of modern phytologists. We are aware, indeed, that his anatomy of the sap-vessels has been denounced as fantastical, and his theory of their origin and functions as absurd; and the anatomy and theory of Kieser extolled and applauded to the skies, as being of the most philosophical character †. Yet we confess that we cannot see the ground whether of this censure, or of this applause. Whatever may be the value or fate of M. Mirbel's theory on this subject, his anatomy was a signal step in advance of all that had preceded it; and if succeeding anatomists have surpassed him in accuracy of research, they have been indebted to his discoveries, or to his errors, for the progress they made. For the rest, what do we find in the greater part of Kieser's sap-vessels but the vessels of Mirbel under a different name? What are we to say of his intercellular canals; and what are we to think of him as an inventor of theories, when we are told that his account of the origin of the cellular tissue surpasses in absurdity even that of M. Mirbel ‡? We have now a more formidable opponent to the theory and anatomy of

* *De Fibræ Vegetabilis Ortu,* sect. i.

† Supplement to *Encyc. Brit.*, p. 289. ‡ *Ibid.* p. 304.

Mirbel than ever Kieser was. It is to the *eclaircissemens* of Dutrochet that we are indebted for any new light that has been lately thrown on the structure of the sap-vessels. Still we cannot do without Mirbel. We must have him, if it were but for the sake of refuting him, as the defenders of our faith must have the books of the infidels.

The vessels of plants are divided by Mirbel, into sap-vessels, and proper vessels. Of the sap-vessels he enumerates five species,—porous tubes, spiral tubes, false spiral tubes, mixed tubes, and cellular tubes—*les vaisseaux en chapelet*. Of the proper vessels he has but one species, which he calls simple tubes.

The first species of tubes containing sap are the porous tubes. They are described by Mirbel as having comparatively a considerable width of diameter, and as being pierced with multitudes of minute holes or pores distributed in parallel and transverse rows. They abound in woody plants. They are not continuous from the base to the summit; but they unite, separate, and unite again, and finally disappear by changing into cellular tissue. The doctrine of porous tubes was attacked by the German phytologists with as much hostility as was that of porous cells. But it met also with some strenuous defenders; and even the antiporists themselves were obliged to admit that the vessels in question are studded with luminous points, or with little transparent vesicles, arranged as the pores are said to be; so that at all events Mirbel's account is not a fiction, but a mistaking of the character of certain given appearances. Thus the matter might have been said to be left in doubt. But as Dutrochet has searched for the pores of the tubes, as he searched for the pores of the cells, without being able to find them; and has, at the same time, adduced strong reasons for supposing them to be merely small and minute molecules imbedded in the substance of the membrane that forms the vessels*, we regard the doctrine of porous tubes as being no longer tenable.

The second species of tubes conducting sap is the spiral tubes—a name not imposed, but adopted, by Mirbel, as being long in use. They are fine, transparent, and thread-like substances, occasionally interspersed with the other tubes of the plant, but readily distinguished from them by their being twisted in the form of a corkscrew from right to left as in the stem of Spearmint; or from left to right as in the stem of Fuller's Teasel. Grew and Malpighi, who first discovered and described them, represented them as resembling in their

* *Recherches Anatomiques*, p. 27.

appearance the *tracheæ* of insects, and designated them by that term, an appellation by which they are still very generally known.

They do not occur often in the root, or at least they are not easily detected in it; yet Kieser is said to have found them in it in great abundance*. Dutrochet could find none, and denies their existence in the root expressly†. Neither are they to be found in the branch or stem of woody plants, except in the annual shoot; nor in the pith of any plant, except in the several species of *Nepenthes*‡. But in the stem and branches of herbaceous plants they are generally to be found without much difficulty accompanying the longitudinal fibres, and forming part of the bundles. They are very easily detected in the foot-stalk, whether of the leaf or flower. The leaf-stalk of the Artichoke affords a good example, in which they are not only remarkably large and distinct, but also remarkably beautiful. They are discoverable also in the leaf itself; more rarely in the calyx and corolla; and more rarely still in the other parts of the flower. Grew and Malpighi found them both in fruits and seeds. I have myself met with them in the external *umbilicus* of the Cherry at a very early period of its growth, that is, about the time of the falling of its petals, but not in any other fruits.

Is it certain that the organs now under consideration are tubes? In the closely coiled up state in which they exist in the growing plant they evidently constitute tubes by the union of their spires. Divide a leaf-stalk of the Artichoke or of the Elder longitudinally, cutting it partly, and tearing it partly asunder, and place a portion of it under the microscope. Inspect the exposed surface carefully and in a strong light, and it will present to your view bundles of spirals in their coiled up and united state. Divide a portion of the same leaf-stalk transversely, cutting it partly and breaking it partly asunder. Place it under the microscope in a clear and strong light, and multitudes of spirals, not yet divided, but merely drawn out and uncoiled, will be found to connect the fractured surfaces; and if you stretch them even till they give way, the fragments will, as if by an elastic and inherent spring, coil themselves up again nearly as before.

Much has been said with regard to their functions. Some have believed them to be air-vessels; others, as Mirbel, regard them as being sap-vessels. Dutrochet believes them to be conductors, not of sap, but of a diaphanous and peculiar fluid, the product of insolation. This we state, however,

* Suppl. Encyc. Brit., Veg. Anat.

† *L'Agent*, p. 20.

‡ Phil. Mag., Oct. 1832, p. 317.

merely *en passant*, as the functions of the several organs of the plant do not come within the scope of our present inquiry.

The third species of tubes conducting sap is the false spiral tubes. According to Mirbel they derive their spiral appearance merely from being cut transversely by parallel fissures. They cannot, like the true spirals, be uncoiled, and are hence fitly denominated false spirals. They are said to abound in the *Lycopodia* and in Ferns, and in the soft parts of the Vine. But what says Dutrochet? He says that what Mirbel took to be parallel fissures are merely globules arranged as in the porous tubes; that the spires may be uncoiled by means of the long-continued action of nitric acid destroying the bond of agglutination; and that the vessels in question are, after all, veritable spirals*.

The fourth species of tubes conducting sap is the mixed tubes, that is, tubes combining in a single individual two or more of the foregoing species. Mirbel exemplifies them in the case of the Flowering Rush; but Dutrochet says there are no such vessels as mixed tubes in any plant whatever, unless you choose to regard as such tubes having some globules arranged in transverse rows, and some scattered. Yet these accidental varieties are not enough to constitute species.

The fifth species of tubes conducting sap is the cellular tubes, that is, tubes composed of a succession of elongated cells united like those of the cellular tissue. Individually they may be compared to the stem of the grasses, which is formed of several internodia separated by transverse diaphragms; and collectively, to a united assemblage of parallel and collateral reeds. Dutrochet admits the existence of such vessels, but he designates them by a different name, and finds them assuming not merely a longitudinal but also a transverse direction, making part of the medullary rays. Like the cells out of which they are formed, they are furnished with globules.

The only species of proper vessels that M. Mirbel describes is the simple tubes. They are the largest of all vegetable tubes, and are formed of a thin but entire membrane, without any perceptible fissure or disruption of continuity. They abound chiefly in the bark, but are found also in the alburnum and even in the matured wood. They convey the descending and elaborated juices. Dutrochet has vessels for this purpose also, but he does not give them the same name nor structure; and he has proper vessels likewise, but he does not assign to them the same function. Hence there is nothing in M. Mir-

* *Recherches Anatomiques*, sect. i. p. 18.

bel's account of the vessels of plants that Dutrochet has not got something to object to.

But as M. Dutrochet may be thus said to have demolished the superstructure of M. Mirbel, it was but fair that he should substitute something in its place. This he has accordingly done upon the firm and legitimate basis both of observation and experiment; so that instead of the vessels of Mirbel, which seem to have been multiplied into too many species, we have now the following, as exhibited by Dutrochet*. 1st. Globule-bearing tubes, or lymphatics, *les vaisseaux corpusculifères.* They abound in the wood, and particularly between the concentric layers of different years. They are studded with small globules, or molecules, that are imbedded in their walls, and they conduct the ascending sap. 2dly. Spindle-shaped tubes, or clostres. They consist of a longitudinal succession of small tubes, swollen in the middle and pointed at the extremities, and often divided by transverse diaphragms. They are destitute of globules. They conduct the cambium, or descending and elaborated juice, partly through the channel of the alburnum, but chiefly through that of the bark. 3rdly. Spiral tubes, or tracheæ. These Dutrochet describes as approaching pretty nearly to the true spirals of Mirbel, with the exception of their being furnished with globules ending in conical spires; but he assigns to them a different function, namely, that of conveying to the interior of the plant a vivifying fluid, the product of insolation. 4thly. Proper vessels, *les vaisseaux propres.* They are found both in herbaceous and woody plants. They have a great comparative width of diameter. They convey the descending and secreted fluids, such as the milky juice of the Fig or Spurge, and yellow juice of the Celandine, and differ from the lymphatics chiefly in being destitute of globules. 5thly. Articulated cellular tubes. They consist of a series of united cells assuming a tubular form, sometimes longitudinally, and at other times transversely, forming the divergent layers. They are regarded by Dutrochet as an emanation from the pith. They seem to correspond to the cellular tubes of Mirbel; but in place of being furnished with pores, we are to regard them as being furnished with globules.

Thus we have reached and inspected the ultimate and elementary organs of which the whole fabric of the plant is composed; and if it is asked of what the elementary organs are themselves composed, the reply is, they are composed, as appears from the same analysis, of a thin, colourless, and

* *Recherches Anatomiques,* sect. i.

transparent membrane or pellicle, fine as the film of a single epidermis, in which the eye, aided by the assistance even of the best glasses, can discover no traces whatever of organization; or if any further analysis is practicable, it is that only which conducts us to the molecules of modern philosophers.

In the course of the foregoing investigations we have seen the great variety of modes in which the elementary organs are grouped, so as to constitute a class, or order, or division. We have seen that these groupings are constant in the orders or divisions in which they are found, and that the greater the complexity of organization, the higher the rank of the plant, as ascending from the lowly Flag or Fungus, to the tall and spreading tree. It is upon this ground that we have instituted our general distribution of plants into Perfect and Imperfect, with the various intermediate gradations which they exhibit; and if the two classes are compared together, the superiority of the former will be rendered manifest, whether we regard their external fabric or their internal structure. Perfect plants are furnished with roots, by which they fix themselves firmly to a spot and absorb nourishment from the soil. Many of the imperfect plants are altogether rootless, and live only by imbibition from the atmosphere, or they are apparently a mere root, and are lodged wholly in the earth. Perfect plants are furnished with a stem and branches, giving them elevation, and expansion, and magnitude. Many of the imperfect plants are altogether stemless, and are scarcely elevated above the level of the soil on which they grow. Perfect plants are furnished with leaves and conspicuous flowers, as well as with conspicuous fruit, giving verdure and odour and beauty to the forest and to the plain, and filling the land with plenty. Many of the imperfect plants are destitute of leaves entirely, and almost all of them are destitute of conspicuous flowers as well as of conspicuous fruit, presenting to the beholder nothing that is showy, nothing that is attractive, nothing that is calculated to delight the eye. Such are the distinctions arising from the external structure of plants. Their anatomy gives a similar result. In the highest orders of vegetables you have the greatest complexity of internal structure, the vascular and the cellular forms being united, and the caudex composed of epidermis, bark, wood and pith, exhibiting an intricate plexus of concentric and divergent layers, cortical as well as ligneous. In the secondary orders you have less complexity of structure, the vascular and the cellular forms being indeed united, and the caudex composed of epidermis, pulp, and interspersed fibre, but exhibiting no plexus of concentric or divergent layers, whether cortical or ligneous. In the lowest orders

you have the least complexity of structure, that is, a structure simply vascular, the caudex being composed merely of a homogeneous mass of pulp covered with a fine epidermis.

Yet the plants called imperfect, though occupying the lowest grade in the scale of terrestrial life, are precisely what they ought to be, and are altogether indispensable to the integrity of the vegetable kingdom. They are, as it were, the foundation on which the superstructure of a more luxuriant vegetation is raised, whether we regard them as having made part of a former world, or as they now exist. They sow their seeds spontaneously. They germinate where no other plants could live. They grow up, and come to maturity, and perish where they grow, and thus form in the process of years a soil fit for the habitat of plants of a nobler order. Hence, though they are devoid of fair forms, they are by no means devoid of utility. Without them we do not know that the more perfect plants could exist: and what would the surface of this earth be if stript of its vegetation?—a mere expanse of desert, or of bare and flinty rock; or, as in the deluge of old, a vast and trackless deep, rolling in unrestrained majesty, and rendering the encompassed globe totally unfit for the habitation of men or of animals; a blank, if we may presume to say so, in the creation of God!

> Quaque fuit tellus, illic et pontus et aer.
> Sic erat instabilis tellus, innabilis unda,
> Lucis egens aer.—OVID, *Met.*, i. 15.

But look at its beautifully diversified surface under the modification of hill and dale, mountain and valley, river, lake, ocean; the hills covered with verdure, the valleys abounding in corn, and in the fairest forms of vegetable being; the mountains skirted with the sturdy oak, the noble cedar, or the lofty palm; the rivers carrying health and fertility along with them in their winding course, and an ocean wafting the tall and stately bark, richly laden with merchandize, to the remotest shores of the habitable globe, and enabling the adventurous mariner to carry back in return the wealth or the superfluities of distant regions.

> Vela damus, vastumque, cava trabe, currimus æquor.
> *Æneid*, iii. 191.

Look at these providential arrangements resulting from the bounty of a kind Creator, and you have a world replenished with everything that is needful to the support of animal life, and paradise fitted for the reception of man!

Ashford, Jan. 17, 1834. P. KEITH.

XLII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 230.]

(*Abstract of Mr. Murchison's Paper "On certain Trap Rocks in the Counties of Salop, Montgomery, &c.," read May 21st,—continued.*)

Brecknockshire.—A RIDGE of trap rock of about three miles in length and half a mile in width, extends from the stream of Nanteinon on the north-east to the right bank of the Yrfon near Llanwrtyd on the south-west. The gorge, in which the little river Cerdin flows, separates this ridge into two mountains, Gaer *Cwm* and Carn Dwad.

The predominant character of the trap is porphyritic, including greenstones, compact felspar rocks, &c. Some of the porphyry is columnar. This nucleus is irregularly coated over with a thin and broken covering of highly altered grauwacke schist, which is frequently in the state of Lydian stone, from beneath the dislocated beds of which bosses of trap protrude. Other varieties of altered rock in contorted positions are seen on the flanks of the ridge, in some of which are crystals of iron pyrites and small portions of carburet of iron.

Absurd trials for coal, similar to those previously described, have been made on the sides of these hills. The mineral source of Llanwrtyd issues from the pyritized schist on the banks of the Yrfon, and it was from a conviction that the phenomena in this case might be due to volcanic action similar to that noticed in the cases near Llandrindod and Builth, that the author was induced to examine this remote district. The result justified his anticipations, and led him to the discovery of this intrusive ridge.

Caermarthenshire.—The only example of trap rock in the large portion of Caermarthenshire which has been examined, was detected last summer in the small rocky knoll of Blaen-Dyffrin-garn, about three miles south-east of Llangadock. This trap is more or less porphyritic, from which state it passes into a rock having a base of compact felspar in parts containing concretions and disseminated green earth. It throws off strata of sandstone of about the same age as those found on the sides of the Wrekin and Caer Caradoc, and the phenomena resulting from the contact are identical with those of Shropshire, the sandstone being converted into a hard and granular quartz rock, having a conchoidal fracture.

This line of altered rocks runs precisely parallel to the strike of the strata of the grauwacke series, and is traceable for miles to the south-west, the most prominent elevation of the quartz rocks being Cairn-goch, formerly a Roman camp.

Small metalliferous veins, chiefly of lead, have been found along this line of eruptive elevation, particularly on the right bank of the river Sowdde.

The only lead mines now in work in Caermarthenshire are at Nant y Moen, seven miles north of Llandovery, and it is highly worthy of remark, that although these are situated so far to the

westward of the range of fossiliferous grauwacke, their relations are analogous to those of the Shelve district, in presenting on one flank a mountainous wall of highly altered conglomerate and quartz rock running from north-east to south-west, which forms the chief rider to those veins, as in the case of the Stiper Stones in Shropshire.

II. *Malvern and Abberley Hills.*—The author highly commends the description of the Malvern Hills published by Mr. Horner in the Geological Transactions *, and refers to it for a faithful account of their mineralogical structure. The present memoir dwells more at length on the effects produced by these sienitic rocks upon the grauwacke strata among which they have been protruded, and which have been altered into chloritic and micaceous schists, highly indurated grits, &c.; whilst others, although unaltered, have been thrown into an inverted position, as pointed out upon a former occasion †.

The author points out the existence of certain bosses of sienite and greenstone in Cowley park to the north of the main chain. He then proceeds to indicate the various points at which masses of eruptive rock hitherto unnoticed appear at the surface in the northern prolongation of the direction of these hills, and connect the Malvern with the Abberley Hills. These are the hills of Berrow, Woodbury and Abberley. They are all of rounded forms, and so covered with vegetation that their structure can only be ascertained from the knobs of rock which occasionally protrude. They chiefly consist of concretionary, compact felspar, sometimes containing crystals of common felspar. That they have, however, a mineral nucleus similar to that of the Malverns, is to be inferred from two observations: 1st. The discovery at the north end of Berrow Hill of a small boss of rock having quite a granitoid character, being made up of compact felspar, quartz and silvery mica, and therefore undistinguishable from a sienite of the Malverns. 2ndly. The discovery of a remarkable dyke at Brockhill, on the right bank of the river Teme, Worcestershire. This dyke is evidently a spur from the trappean hills of Woodbury and Abberley, from the first of which it is distant only 1½ mile, being thrown out in a direction from east to west. The dyke cuts through the old red sandstone, and consists chiefly of sienite or sienitic greenstone perfectly analogous to many varieties of the Malvern Hills. It is in great part columnar, the ends of the columns being at right angles to the walls of the dyke. The strata in contact with the trap are much hardened; the mica appears to be driven off from the sandstone, the colour of which is changed to a dingy dark purple; and the associated marls and cornstone are converted into hard amygdaloids, with veins of carbonate of lime, crystals of iron pyrites, &c. &c.

Another dyke of trap traversing the old red sandstone, which is marked in Mr. Greenough's map, occurs at Bartestree near Hereford. It is a basaltic greenstone, with olivine, and the strata in contact are affected similarly to those of Brockhill near Abberley.

III. *Trap Rocks penetrating the Coal Measures.*

* Geol. Trans., 1st Series, vol. i. p. 281.

† Proceedings, vol. ii. p. 18: and Lond. and Edin. Phil. Mag. and Journ., Third Series, vol. iv., May 1834, p. 375.

Clee hills.—The basalt of the Titterstone and Brown Clee Hills having been described, the author only refers to his former communications for the purpose of stating, that Mr. Lewis of Knowlbury has proved the existence of an eruptive wall of basalt which cuts off the coal of the Treen pits, precisely in the linear direction of that which has been called the Jewstone (basaltic) fault. The coal in the proximity of the basalt is much altered and of no value.

Wenlock and Coalbrook Dale.—The greenstones and amygdaloids which rise up at various points through the carboniferous limestone and coal measures of this tract, are marked by the author on the Ordnance map, but he suggests that their chief interest will be apparent when the survey of all the dislocations of these coal fields by Mr. Prestwich, shall have been completed.

Kinlet.—This is rather a peculiar trap, being a greenstone in which white spots of granular felspar are dotted through a base of dark hornblende. It rises into knolls, protruding through the coal measures, flanked by old red sandstone.

Arley and Shatterford.—This dyke of trap has been described by the Rev. J. Yates*, to which account the author adds some details respecting its structure, and the beds of coal, sandstone and shale upon its sides; the chief additional fact being, that the red sandstone which immediately surrounds this narrow zone of coal measures is not the *new* red sandstone of Bewdley and Kidderminster as formerly supposed, but a girdle of *old* red sandstone, with beds of cornstone, which is distinctly separated from the new red sandstone and folds round this peninsulated carboniferous tract.

Conclusion.—From the natural phenomena described in the preceding pages it appears,

1. That volcanic agency satisfactorily accounts for the appearance of all the varieties of trap rock which are associated with the grauwacke series, the old red sandstone, and carboniferous strata in the country under review.

2. That from the imperceptible passages which take place between the different varieties of these trap rocks it is difficult, from mere lithological characters, to assign a separate age to each.

3. That as some of the porphyritic and felspathic rocks, alternate conformably with strata of marine origin containing organic remains of a very early period, and as some of the layers in which such remains are imbedded have a base of true volcanic matter, the date of the origin of this class of rock is thereby fixed.

4. That these conformable alternations of trap and marine sediment establish a direct analogy between their mode of production and those replications of volcanic ejections and marine deposit which are now going on beneath the present seas; whilst they further explain the manner by which, in times of the highest geological antiquity, the porphyry slates were arranged in parallel laminæ with the sedimentary accumulations of that age.

5. That the existence of certain strata containing organic remains, yet possessing a matrix, composed in great measure of the same materials as the adjacent ridges of trap rock, has strengthened

* Geol. Trans., New Series, vol. ii. p. 249.

the inference that some of the ebullitions of these submarine volcanos were contemporaneous with the period in which these animals lived and died, the finer volcanic ejections having, it is presumed, led to the formation of the volcanic sandstone.

6. That subsequent to these contemporaneous classes, other trap rocks have been forcibly intruded, amidst deposits of all ages, from the oldest grauwacke up to the strata of the coal measures, producing great derangement, fracture, and alteration in the beds which they penetrate, exhibiting, at the points of contact, siliceous schists, porcellanite, many crystallized substances, veins, quartz rock, &c.

7. Special attention is invited to that change by which sandstone has been converted into quartz rock, because it appears to explain how the so-called *primary* quartz rock may have been formed.

8. It is inferred that all the metalliferous veins in the country described are due to the presence of the contiguous trap rocks, and that therefore this inquiry has amply corroborated the theoretical speculations of M. Necker*.

June 4.—A paper was first read, entitled, "Observations on the Strata penetrated in sinking a Well at Diss, in Norfolk," by John Taylor, Esq., Treas. G.S.

The well alluded to in this communication affords the only details, hitherto made public, of the thickness and character of the chalk in that part of Norfolk in which Diss is situated.

The well was sunk by Mr. Thomas Lombe Taylor, and the following list gives the order and thickness of the beds:

Clay	50 feet.
Sand	50 —
Chalk, without flints, soft and of a marly nature	100 —
Chalk, with flints in layers of single stones, distant about a yard from each other	330 —
Grey chalk, with an occasional layer of white chalk, and free from flints	60 —
Light bright blue chalk, approaching to clay, with white chalk stones	20 —
Sand	5 —
	615

On penetrating the light blue chalk, the tools sunk rapidly for about 5 feet, and the water rose to within 47 feet of the surface, at which height it is stated to have continued.

A paper, entitled, "Observations on a Well dug at Lower Heath, on the South Side of Hampstead," by Nathaniel Wetherell, Esq., F.G.S., was next read.

The strata penetrated in making this well are stated by Mr. Wetherell to be as follows:

London clay	285 feet.
Rock	5 —
Plastic clay	40 —
	330

* Proceedings, vol. i. p. 392: and Lond. and Edin. Phil. Mag. and Journ., Third Series, vol. i., September 1832, p. 225.

The London clay, for the first thirty feet, was of a loose texture, reddish brown colour, and contained a good deal of iron pyrites and selenite: for the next hundred and seventy feet it varied in colour from blue to dark brown, and contained many septaria; and the lower part was very sandy. At the depth of 260 feet a few fruits and seeds were procured,—the former resembling those found at Sheppey, and the latter those found at Highgate; but between 265 and 285 feet the clay abounded with vegetable remains. A classed list is given of the fossils obtained by the author; and among those not previously noticed, he mentions the remains of Asterias, a Pentacrinite, six species of bivalves, and two small, straight, tubular bodies, one round, the other square, having an internal radiated structure like that of a Belemnite, but without a central cavity.

The rock between the London and plastic clays was full of green particles, and contained numerous rounded flint-pebbles. The fossils obtained from it were chalky and friable, and among them the author found *Mya intermedia* and *Natica glaucinoides*, shells characteristic of the Bognor rock.

The plastic clay presented its usual mottled appearance, but no organic remains were noticed in it.

At the depth of 330 feet a bed of sand, containing small flint-pebbles, occurred, and the water gradually rose from it to within 200 feet of the surface.

A letter was afterwards read from Sir Philip Grey Egerton, Bart. F.G.S. to the Rev. Prof. Buckland, D.D., F.G.S., "On the Ossiferous Caves of the Hartz and Franconia."

This letter contains a detailed enumeration of the remains found by Sir Philip Egerton and Viscount Cole in the caves of Gailenruth, Kühloch, Scharzfeld, and Baumanns Höhle. In communicating these results, the author states that he has made the list as complete as possible, as from the extreme strictness with which the Müggendorf caves are now guarded, it is not probable that any large collection of bones will again find its way to this country. With respect to the caves of Kühloch and Rabenstein, he states, that the account which he gave in 1829 of their destruction *, has been fully verified by Lord Cole, who visited them last year; but in consequence of the absence of the Baron of Rabenstein at the time of their destruction, the author acquits him of having been implicated in the transaction; and adds, that this nobleman has proved himself a strenuous friend and patron of science, by the care with which he protects the newly-discovered cave of Rabenstein from depredation.

The author states that he found recent bones of pigs, birds, dogs, foxes, and ruminantia, in every cave which he examined; fragments of rude pottery in those of Scharzfeld, Baumanns Höhle, Gailenruth, and Zahnloch; and old coins and iron household implements of most ancient and uncouth forms in that of Rabenstein. In the Gailenruth cave he did not find one bone gnawed by

* Phil. Mag. and Annals, N.S., vol. vi. p. 92, 1829; and Quarterly Journal of Science, vol. vi. p. 213, 1829.

hyenas, but numerous bones of bears marked with short scratches, the effects, he conceives, of their having been the playthings of young bears; an idea first suggested to him by observing the amusement a ball of wood affords a bear in confinement.

Of the genus Ursus, Sir Philip Egerton states that he had found among the remains procured in the Gailenruth cave, two bones of the carpus, one of the metacarpus, one of the metatarsus, and part of the sternum; all required to complete Cuvier's account of the osteology of the animal; that in the collection from the same cave, he had found the large incisor of the right side of nearly ninety bears, and of all ages, from the cutting-tooth to the worn-down stump; that he had not a single humerus out of the many procured from various localities, with the perforation at the condyle for the cubital artery; and that out of 36 specimens in his collection, besides a greater number in Viscount Cole's, he had not noticed one containing the least trace of the small anterior false molar.

Sir Philip Egerton then gives a detailed list of every bone procured by himself and Lord Cole. This list it is impossible to give in an abstract; but it may be stated that the bones obtained from the cave of Gailenruth belong to *Felis Canis*, *Canis Vulpes*, Hyæna and Gulo; those from Kühloch to Hyæna, *Canis Vulpes* and Rhinoceros; those from Scharzfeld to Felis and Canis; and those from Baumanns Höhle to Felis.

A letter was also read from Hugh E. Strickland, Esq. to G. B. Greenough, Esq., P.G.S., "On the Occurrence of Freshwater Shells, of existing Species, beneath the Gravel near Cropthorne, in Worcestershire."

In a former letter, dated September 28th, 1833, Mr. Strickland stated that he had found in a gravel-pit near Cropthorne, fresh-water shells of existing species, but that the circumstances under which he had noticed them induced him to hesitate in assigning them to the age of the gravel. The surveyor of the roads having, however, at the request of the Worcestershire Natural History Society, opened a new pit, Mr. Strickland is enabled in this letter to state, that the same species of shells had been found under horizontal beds of gravel and sand, which presented no sign of having been disturbed since their deposition. He also states that the remains of the hippopotamus, deer, and he believes ox, had been found in considerable abundance in the same pit.

A notice "On the Action of High Pressure Steam on Glass and other Siliceous Compounds," by Prof. Turner, M.D., Sec. G.S., was then read.

An opportunity having presented itself to the author, of including substances in a high pressure steam-boiler, he took advantage of it to try the effect which would be produced on glass; and he accordingly encased in wire gauze some specimens of plate- and window-glass, and suspended them from the top of the boiler, so that they were surrounded by steam whenever the boiler was in action. They were kept in this situation for four months, during

which time the boiler was commonly in action ten hours daily, except Sundays, its temperature being then at 300° Fahr. On opening the boiler at the end of the time specified, all the pieces of glass were found to have been more or less decomposed; and the plate-glass in particular, which is a glass of silex and soda, was far advanced in decomposition. Flat pieces, $\frac{1}{8}$th of an inch thick, were in some parts decomposed through their whole substance; while in others a layer of unchanged glass was found in the middle, covered on each side with a stratum of opaque white siliceous earth, having the appearance of chalk.

The author referred these changes to the influence of water on the alkaline matter of the glass. The white earthy portions were found to be entirely free from alkaline matter, which had been dissolved and removed by the water which condensed upon the glass at the successive heating and cooling of the boiler, or which may have been thrown upon it by splashing during ebullition. But the author considered that the actual loss was not due to the extraction of alkaline matter only, but that the silex of the glass had in some measure been dissolved along with the alkali. This was proved to have been the case by the apertures of the gauze envelope being filled up at the most depending parts by a siliceous incrustation, where also a stalactitic deposit of silica, about 1$\frac{1}{4}$ inch long, had formed.

A piece of window-glass included at the same time with the plate-glass, was also in a decomposing state, but in a much lower degree. A piece of rock-crystal confined in the boiler at the same time was wholly unchanged.

The author adduced these facts as illustrative of the action of water at high pressures on felspathic and other rocks containing alkaline matters.

LINNÆAN SOCIETY.

April 15.—The reading was commenced of a paper containing "Observations on some species of native Mammalia, Birds, and Fishes, including additions to the British Fauna." By William Thompson, Esq., V.P. Belfast Nat. Hist. Society.

The author commenced by stating, that a perusal of the Rev. Mr. Jenyns's paper, entitled "Some Observations on the Common Bat of Pennant, with an attempt to prove its identity with the Pipistrelle of French authors," (Linn. Trans., vol. xvi.) induced him to examine specimens of the common bat of the North of Ireland, which hitherto, like that of England up to the period of Mr. Jenyns's paper, has been considered the *Vespertilio murinus* of Linnæus, as well as of recent continental authors.

This examination led to the same conclusion as that of Mr. Jenyns, the common bat of Ireland proving identical with that of England, and consequently with the *V. pipistrellus* of the Continent.

Observations on the habits, &c., of this species, when at large and in captivity, were also given in detail, and were followed by some

remarks on the Long-eared Bat (*Plecotus auritus*) as observed in Ireland.

The occurrence of the *Larus Sabini* in Ireland on two occasions was next adverted to. Of this bird two specimens only had previously been recorded as met with in the eastern hemisphere, both of which were obtained by Captain Sabine at Spitzbergen. The specimens which formed the subject of the present paper were rendered peculiarly interesting from being in the plumage of the first year, in which state the *Larus Sabini* had not before come under the inspection of the naturalist. The appearance presented by the species at this age was described with great minuteness, and also the differential characters by which it may at all ages be distinguished from its congener the *Larus minutus*.

The specimens described are contained in the Museums of the Natural History Society of Belfast and the Royal Society of Dublin.

From the examination of a specimen of the *Cygnus Bewickii*, killed in the North of Ireland, and preserved in the Belfast Museum, the author stated that he was led to discover that some of the characters by which this species has hitherto been distinguished are erroneous.

The principal character pointed out as such was the number of *rectrices*, or tail-feathers, which are described in the Linn. Trans. (vol. xvi. p. 445, *et seq.*), Illus. of Orn. (Part 6), Illus. of Brit. Orn., &c., to be 18, though they are in reality 20. The correctness of the view respecting this and the other characters thus dwelt upon was subsequently confirmed from an inspection of two living birds, which have been since Feb. 1830 in the possession of William Sinclaire, Esq., of the Falls, near Belfast.

Observations on the disposition, habits, &c., of these individuals were also added.

Mr. Thompson embraced this opportunity of exhibiting a specimen of the *Potamogeton prælongus* of Reichenbach, with which he had been favoured by William Henry Harvey, Esq., of Limerick, who had the satisfaction of discovering the plant last autumn in the river Shannon, and thereby making a very desirable addition to the British Flora.

May 6.—The reading of Mr. Thompson's paper was resumed and concluded.

The Three-spined Stickleback of the North of Ireland was dwelt upon at considerable length, and the differential characters between it and the three English species, as described by Mr. Yarrell (Mag. of Nat. Hist., vol. iii.) pointed out. From all of these it was stated to be distinct, but seemingly identical with the *Gasterosteus brachycentrus* of the *Histoire Naturelle des Poissons* of Cuvier and Valenciennes, (tom. iv. p. 499. pl. 98.) a species there published as new, and mentioned as having been obtained by M. Savigny from the brooks of Tuscany.

The discrepancy between Cuvier (*Règne Animal*, 2nd edit.) and British authors relative to the *Gasterosteus pungitius* of Linnæus was next noticed.

It was remarked of the *Gobius niger* from specimens taken in the

North of Ireland, (on the shores of which country the species has not before been recorded as met with,) that the fish so named by Donovan, with which these were identical, is distinct from the *G. niger* of Pennant, and as such ranks as a third species of *Gobius* to the British Fauna, two species only having yet a place in it.

The *Cyclopterus Montagui* (Don.), which stands recorded as having been taken only on the southern coast of England, and there but by its discoverer, was next introduced, from the circumstance of a specimen occurring to the author on the coast of the county of Down in December 1833. The difference, consisting chiefly in colour and markings between this fish (which was mature) and Col. Montagu's as described in the Wern. Mem. (vol. i. p. 92.), was pointed out.

Specimens of all the species treated of in this paper, with the exception of the *Cygnus Bewickii*, were exhibited.

Mr. Thompson further announced the following species of land and freshwater shells to be indigenous to Ireland, though they have not been recorded as such. They do not appear at least in any of the three published catalogues of the shells of that country, nor is he aware of their being incidentally noticed elsewhere.

They were brought forward on this occasion preparatory to a general catalogue of the land and freshwater shells of Ireland, with observations on the relative abundance and scarcity of the species, their geographical distribution, &c., which Mr. T. hopes to lay before the Society early in the ensuing year.

Pisidium nitidum, Jenyns. *P. pulchellum*, Jenyns. *Limacella Parma*, Lister. *Testacellus Scutulum*, Sowerby. *Vitrina Draparnaldi*, Jeffreys.—*Helix hortensis*, Montagu. This species and the *Helix aspersa* do not both appear in any catalogue of Irish shells. Capt. Brown enumerates the *H. hortensis* in his "Irish Testacea," but his reference to Donovan (t. 131.) makes it evident that it is the *H. aspersa* of Montagu, which he brings forward under the name of *hortensis*.—*H. hispida*. In the "Irish Testacea" the *Helix hispida* is described to be that of Montagu, and of Donovan also; but as the same name is applied to two different species by these authors, and as one species only has yet been recognised as found in Ireland, I now add the second, as I am in possession of Irish specimens of the *hispida* of both authors.—*H. spinulosa*, Montagu. *H. lucida**, Draparnaud. *H. nitidula*, Draparnaud. *H. alliaria*, Miller. *H. crystallina*, Drap. *H. radiatula*, Alder. *H. pura?*, Alder. *H. pygmæa*, Drap. *H. Scarburgensis*, Bean. *Clausilia bidens*, Drap. *Carychium lineatum*, Ferussac. *Bulimus Acicula*, Drap. *Pupa edentula*, Drap. *P. muscorum* (a.), Drap. *P. Secale*, Drap. *P. pygmæa*, Drap. *P. Antivertigo*, Drap. *P. Vertigo*, Drap. *P. sexdentata*, Alder. *P.* (*Turbo*) *Vertigo*, Montagu. *P.* (*Vertigo*) *Anglica*, Ferussac. *Planorbis spirorbis*, Drap. *P. glaber?* Jeffreys. *P. lutescens*, Lamarck, or *P. carinatus*, Drap. As in the case of the *Helix hortensis* and

* This species should not perhaps be included here, as Mr. Jeffreys remarks of it, under the title of *H. nitida*, that he has received a specimen of a variety of this shell from Mr. Dillwyn as Irish.

Helix hispida, the synonyms of two distinct species are in the "Irish Testacea" again appropriated to one. These are the *Helix planata* of Maton and Rackett and the *H. carinata* of Montagu, and as these have not severally been given in any catalogue of the shells of Ireland, and my cabinet contains specimens of both species collected there, one or other, the *Planorbis lutescens* of Lamarck, or *P. carinatus* of Draparnaud (to adopt modern names), remains now to be added.—*Valvata Planorbis*, Drap.

A beautiful species of *Limneus*, collected in the South of Ireland by William Henry Harvey, Esq., of Limerick, and presumed to be new to science, was also characterised by Mr. Thompson. It approximates the *L. glutinosus* more nearly than any other British species of this genus.

A notice of an addendum to this paper subsequently read to the Society will be found in our Number for July, p. 70.

ROYAL ASTRONOMICAL SOCIETY.

April 11.—The following communications were read:—

I. Places of the periodic Comet of 6·7 years, or Comet of Biela, deduced from the Observations of Sir John Herschel at Slough, and of Mr. Henderson at the Cape of Good Hope. By Mr. Henderson.

The positions of the stars with which the comet was compared were obtained partly from the catalogues of MM. Pond and Bessel, but chiefly from observations made at the Cape Observatory. Founded on these determinations, there are given—the comet's apparent geocentric right ascension and declination on September 23rd and 24th, and November 3rd and 4th, 1832, deduced from Sir John Herschel's observations—and on November 18th, 22nd, 23rd, and 27th, December 26th and 31st, 1832, and January 1st, 2nd, and 3rd, 1833, deduced from the observations of Mr. Henderson.

II. Various Observations made at the Observatory, Cape of Good Hope, in April and May 1833. Communicated by Mr. Henderson.

1. Observed Transits of the Moon and Moon-culminating Stars. By Lieut. W. Meadows, R.N.

2. Observations of the Moon and Stars made with the Mural Circle, on April 29th, 1833.

3. Observation of a Lunar Occultation of η *Geminorum*, made on April 24th, 1833.

This additional series of observations made at the Cape Observatory by Messrs. Henderson and Meadows, is a continuation of those already communicated. (See *Monthly Notice* for June 1833.)

III. Results of a comparison of various Observations of *Mars* made at Cambridge, Greenwich, and the Cape of Good Hope, for the determination of the Sun's Parallax. By T. Henderson, Esq. In a Letter to Professor Airy, dated Nov. 11th, 1833.

Mr. Henderson has compared various observations made at Cambridge, and at Greenwich with both circles, with his own made at the Cape of Good Hope; and has deduced the correction of the declination of *Mars*, as given in the *Berlin Ephemeris*. The results

are as follow :—Supposing the assumed solar parallax, $8''{\cdot}578$, to be corrected as in the *Cambridge Observations for* 1832, pages 138 and 139, so that the true parallax is $8''{\cdot}578\ (1+\mu)$, the value of μ is determined as follows:—

Compared with the Cape Obs.	Limb.	Value of μ.	Parallax of ☉ from mean of preceding.
Cambridge	North	+ 0·0127	$8''{\cdot}588$
———	South	− 0·0102	
Greenwich (Troughton)	North	+ 0·0895	$9''{\cdot}076$
	South	+ 0·0265	
Greenwich (Jones)	North	+ 0·0632	$9''{\cdot}343$
	South	+ 0·1152	

"The chief inference," Mr. Henderson remarks, "to be drawn from the above observations appears to be, the probable accuracy to be expected from similar observations at future periods."

Mr. Henderson states that the season in which these observations were made was not favourable for delicate astronomical observations at the Cape, as the south-east wind was generally prevalent, during which the atmosphere is in a disturbed state. The season best adapted for accurate observations there is from March to October, or during the winter months.

IV. On a Clock for giving Motion in Right Ascension to Equatorial Instruments. By the Rev. R. Sheepshanks. This paper, illustrated by an engraving, is given in the *Monthly Notices* of the Society.

V. Stars observed with the Moon at Cambridge Observatory, in the month of March 1834. Long. $23^{s}{\cdot}54$ east of Greenwich.

May 9.—The following communications were read :—

1. Second Series of Micrometrical Measures of Double Stars chiefly performed with the 7-feet equatorial. By Sir J. Herschel.

This second series is a continuation from vol. v. p. 90 of the *Memoirs*, and begins with No. 736, the last in the above-mentioned page being 735. The observations have been almost entirely made with the same achromatic of five inches aperture, except in the case of a few difficult objects, for which the 20-feet reflector was used. Many of them, and in particular all those of 1833, were made with an increased magnifying power, which Sir J. Herschel calls 500, but thinks may be something less. The action of the instrument under this power is perfectly good; and, having provided himself with still higher powers, Sir. J. Herschel expects to be able in future to avail himself fully of the excellent quality of the object-glass, which has hitherto become uniformly more apparent with every increase of power which has been applied to it.

Sir John Herschel's observations end at No. 1, 111, and to them are added a few made by Captain Smyth and the Rev. W. R. Dawes.

II. Micrometrical Measures of the Positions and Distances of 121 Double Stars, taken at Ormskirk, in the years 1830–33. By the Rev. W. R. Dawes.

These observations are given in the same manner as those of Sir J. Herschel, and are 406 in number. For a description of the instrument employed, see Memoirs, vol. v. p. 135. Mr. Dawes further

states, that the mounting and entire arrangements of his 5-feet achromatic are exactly similar to those of Capt. Smyth's larger instrument, described in the Memoirs, vol. iv. p. 550. The spherical aberration is perfectly destroyed, and the discs of stars shown with beautiful neatness on a dark ground.

"Since the middle of the year 1831, I have uniformly placed the stars between the parallel threads in measuring their position. This plan, suggested to me by Sir J. Herschel, I much prefer to any other. I can also speak in the highest terms of the advantage accruing from the use of a red illumination of the field; and I usually employ as deep a colour as the light afforded by the lamp will permit. But the most important improvement I have yet tried consists in the interposition of a concave achromatic lens between the object-glass and its principal focus, by which the focal image being enlarged to above twice its original size, a very high magnifying power may be obtained, while the threads of the micrometer appear of sufficient fineness to permit the measurement in distance of very close and minute stars without distortion. Having frequently felt the inconvenience arising from the threads being magnified in proportion to the power used on the telescope, I stated the difficulty to Mr. Dollond, who speedily and most perfectly removed it by the application of the lens above mentioned. The effect is the same, in respect of the power of the telescope and the fineness of the threads, as if the focal length of the object-glass were increased to about 10 feet 2 inches.

"The magnifiers, with the micrometer, have been varied according to the object and the circumstances. At first I usually employed 226; this was subsequently exchanged for 285, which was found more generally efficient; while 55, 80, and 140, were occasionally applied for very faint objects, and 350, 480, 550, and 625, for very close or bright ones. Since the acquisition of the concave achromatic lens, a power of 295 has been most frequently employed, by using the eye-tube which without it magnifies 140 times; but in very unfavourable circumstances, or on excessively faint objects, 170 has been occasionally substituted. The highest magnifier has, however, generally been used, which the state of the atmosphere and the brightness of the object would permit; and great advantage has frequently been derived from the use of 475 or even 600.

"With the five lower eye-tubes a *diagonal prism* can be employed, the highest power to which it was originally applicable being 285; but since with the second object-lens this eye-tube magnifies 600 times, the use of the prism is proportionally extended. And it appears to me that much greater benefit may be derived from this little appendage than merely the increased comfort to the observer, in releasing him frequently from a very awkward position, and placing him in one comparatively luxurious, though this alone exerts no small influence on the correctness of these extremely delicate observations. But I should chiefly recommend the prism to those who are seriously engaged in these researches, as enabling the observer to place the *stars* in any positions he may please with relation

to a vertical circle. Thus he may get rid of one of the principal difficulties in measuring objects correctly, whose position is between 30° and 60° from the parallel, and which, I am persuaded, has had a large share in producing the discordant results in the angles of ϵ *Boötis,* 70 *Ophiuchi,* ζ *Boötis, Canis Minoris* 31, and others. By simply turning round the eye-tube and its annexed prism, we may place such oblique stars either *vertically* or *horizontally,* or we may *invert* their relative situation; and thus, by taking a few observations in each apparent position, much may, I feel convinced, be done towards destroying any bias of judgement which might arise from the oblique position of the stars, or from an unconscious disposition to place the threads of the micrometer in the direction of a *tangent to the discs of moderately unequal stars,* and that principally on *one side* of them."

Measures of distance were usually taken alternately on each side of the zero, instead of employing an index correction. They are in most instances decidedly smaller than those of MM. Herschel and South, Phil. Trans. 1824, and somewhat larger than those of Professor Struve. Distances often vary to an astonishing extent when the stars are well defined: at other times the converse takes place.

A central disc on the object-glass was found to increase the *separating* power of the telescope; but the concentric rings round bright stars were thereby multiplied, made more luminous, and thrown further from the disc.

The estimated weight of each observation was fixed upon before the index was read off; and every suspected observation was re-made. If the second observation decidedly agreed with other measures, the suspected one was rejected; but if not, both were made to form part of the set. All the original observations of the series, 4400 in number, have been preserved.

Mr. Dawes makes several comparisons of his own observations with those of others. With regard to some stars, he has found reason to suspect some alteration; with regard to others, he can give no support to surmises respecting their binary character. Among the stars which furnished the former results, Mr. Dawes mentions *Ceti* 292, γ *Ceti,* Σ 389, *Tauri* 34, 2 *Camelopardalis,* 38 *Geminorum,* H. I. 69, ϵ *Hydræ,* 35 *Sextantis,* 44 *Boötis,* ξ *Libræ,* S. 757, H. 87, *Pegasi* 29, ζ *Aquarii,* Σ 3061; among the latter, 32 *Eridani,* H. I. 70, H. I. 84, H. III. 48, *Canis Minoris* 31, ζ *Boötis,* δ *Serpentis,* Σ 2339, *Tauri Poniat.* 75, *Cygni* 280.

III. List of Observations made with the Transit Instrument, from April 10 to May 24, 1833, and with the Mural Circle, from May 16, 1832, to May 24, 1833, at the Cape of Good Hope. From Mr. Henderson.

The transit observations were made (with a few exceptions) by Lieut. Meadows, and the circle observations by Mr. Henderson.

IV. Apparent Geocentric Positions of Encke's Comet, deduced from observations made by Mr. Henderson and Lieutenant Meadows, at the Observatory, Cape of Good Hope, and by M. Mossoti,

at Buenos Ayres, in June 1832. In a letter from Mr. Henderson to Professor Airy.

These positions have been computed by Mr. Henderson from the observations, and he has compared them with those interpolated from Mr. Encke's accurate Ephemeris, communicated to the Society in May 1832. Mr. Henderson considers, as a definitive result obtained from the Cape observations, that the mean correction of the ephemeris in right ascension is $+2' 25''$ reduced to a great circle, and in declination $+1' 30''$, the mean corresponding date being June 5, 1832.

V. Catalogue of the North Polar Distances of 60 Stars, reduced to January 1, 1830, derived from Observations made at Greenwich by the two circles and six microscopes, 1825–1833. From the Astronomer Royal. An Abstract of this paper is given in the *Monthly Notices.*

VI. Stars observed with the Moon at the Royal Observatory, Greenwich, from February 1833 to March 1834.

June 13.—The following communications were read:

I. Some remarks on the Greenwich mural circles, by the Astronomer Royal.

The memoir lately published by Professor Airy, in which he notices some discordances in the zenith points of the Cambridge circle as determined by different stars observed directly and by reflection, induced Mr. Pond to examine whether one or both the mural circles of Greenwich exhibited the same defects. The observations of 1825 and 1826 being very numerous and carefully made, Mr. Pond has selected a series of differences of declinations between certain stars observed in those years. As the altitude of each star is obtained with each circle by direct and reflected vision, independently, (the stars being in fact observed as church steeples are with a theodolite,) it is evident that there are four combinations of differences between every pair of stars, wholly free from any hypothesis as to the absolute declinations, the agreement or disagreement of which will show the accuracy or defects of the instruments. From the near coincidence of each result with the mean, Mr. Pond concludes that the circles were at that time in a satisfactory state.

II. Observations of the Solar Eclipse, July 16, 1833, at the Cambridge Observatory. By Professor Airy.

Professor Airy considers the beginning or end of a solar eclipse so unsatisfactory an observation compared with the mode adopted by him on the present occasion, that he did not think it worth while to note them at all. The instrument employed was a 5-feet equatorial by T. Jones, magnifying power 100, which has been noticed in the determination of the elongations of Jupiter's fourth satellite and of the parallax of Mars. The only sensible error in its adjustment was that in azimuth, which was unimportant in the present instance, as the middle of the eclipse was almost exactly at 6^h from the meridian. The observations were of differences of N.P.D. and differences of R.A. When differences of N.P.D., as

of two cusps, were observed, the instrument was clamped in R.A., and one of the micrometer wires (this instrument has two), the position of which was previously read off, was brought upon the cusp by the tangent screw of the declination circle, as the cusp passed the centre wire; and the time was noted. The second cusp was bisected by the second micrometer wire when it passed the centre wire, the time being also noted and the micrometer read off after the observation was made. The subjects registered in this observation are the clock times of the passages of the cusps, and the readings of the two micrometers when the wires were placed upon each. The differences of R.A. were obtained by observing the transits of each cusp over all five wires when the interval allowed, and in other cases over the three middle wires. These observations were carefully made on the same part of the wires, the declination circle being unclamped and moved by hand.

"By observing differences of either N.P.D. or R.A. of the cusps near the beginning and near the end of the eclipse, the effects of errors in R.A. and N.P.D. would have been obtained, largely multiplied and combined in opposite ways; and by observing differences of N.P.D. of the cusps near the middle of the eclipse, the effects of errors of R.A. would have been obtained with large multipliers. The only defect in these determinations would be, that, as far as errors in the semidiameters of the sun and moon enter, they always enter with the same signs. There seems to be no good method of finding the effects of errors in the semidiameters combined with different signs, except by measuring the difference of declination of the N. or S. limbs when those of both bodies are visible. It would, perhaps, therefore, have been best to divide the duration of the eclipse into five nearly equal parts, and to observe difference of N.P.D. of cusps during the first, third, and fifth parts, and difference of N.P.D. of limbs during the second and fourth. I am here supposing the object to be (as mine was) to correct all the elements; if the object were only to ascertain differences of longitude (supposing the elements corrected), it would be best to observe differences of R.A. of the cusps during the second and fourth parts, as the whole of the measures thus obtained would vary rapidly with the time. Different considerations will be necessary in every different eclipse; and in none can measures be made to the utmost advantage without more of previous examination than I was able in the present instance to give."

The observations made are,

1. Excess of N.P.D. of 1st cusp over that of 2nd cusp. 6 observations.
2. Excess of N.P.D. of sun's lower limb over 1st cusp. 4 observations.
3. Excess of N.P.D. of sun's lower limb over moon's lower limb. 5 obs.
4. Excess of N.P.D. of 2nd cusp over that of 1st cusp. 10 observations.
5. Excess of R.A. of 2nd cusp over that of 1st cusp. 10 observations.

After pointing out the mode in which the corrections for refraction, parallax, &c., are obtained, Professor Airy proceeds to calculate the small variations depending on the errors of the tables, and to obtain equations of condition involving the corrections of the R.A.,

N.P.D., and semidiameters of the sun and moon. The solution of these equations, on the supposition that the place of the sun in the Berlin Ephemeris is correct, shows that the true R.A. of the moon at the time of the eclipse is less than that of the Berlin Ephemeris by 19″·30, and less than that of the Nautical Almanac by 18″·55, while the true north declination is less than that of the Berlin Ephemeris by 1″·26, and less than that of the Nautical Almanac by 1″·21. These are the corrections which ought to be applied to the tabular values of the moon's place in deducing longitudes, &c., from observations of this eclipse.

III. On the position of the ecliptic, as inferred from transit and circle observations, made at Cambridge Observatory, in the year 1833. By Professor Airy.

Those observations only were employed in which both limbs were observed; giving 140 transit, and 134 circle, observations, six microscopes being read for each limb in the latter. The clock errors were deduced from a catalogue differing from Pond's of 1112 stars by a mean excess of $0^s{\cdot}11$, and from Bessel's in the *Tab. Reg.* by a mean excess of $0^s{\cdot}18$.

In the circle observations, the first limb was observed by setting the instrument approximately, reading the microscopes, and measuring the distance of the limb from the fixed wire by a micrometer wire. The other limb was observed in the usual way. The refractions used were Bessel's, the parallaxes those of the *Berliner Jahrbuch*, both applied separately to each limb. The latitude of the place was derived from 917 observations, with six microscopes, of 10 circumpolar stars. See *Camb. Obs.*, vol. vi. The method adopted, that of *Normal Places*, is thus described:

"In England, in deducing values of the obliquity, or places of the equinox, from a set of observations, the usual method has been to calculate from each observation by a trigonometrical operation the place of the equinox, &c., and to take the mean of all the places so found. The method of *Normal Places* consists in calculating trigonometrically no single observation whatever; but in comparing every observation with the place in the ephemeris, in taking the difference or apparent error of the ephemeris, in taking the mean of all those apparent errors over an extent of time in which we have *à priori* reason to think that they ought not to vary much, and then in considering that we have thus obtained, for the mean of all those times of observations, an error of the ephemeris which is very much more accurate than any one error. By applying this error with changed sign to a place in the ephemeris for a time near to the mean of the times of observation, we obtain a single corrected place, which possesses all the accuracy derived from the mean of numerous observations; and this is a Normal Place. The only requisites for the ephemeris to be used in these calculations are,—that it be consistently and accurately calculated on some elements, and that these elements be not extremely far from the truth (their being very near to it is of no importance whatever). We may now, if we please, use the Normal Places for trigonometrical calculation,

or we may use the mean errors as the errors for the mean times, and make our whole calculation one of errors."

By grouping the observations for each month, in the manner above described, Professor Airy deduces from the comparison of the errors which are given, that his observations (his own catalogue of stars included) give the sun north of the ecliptic assumed in the tables, by

$$-1''\cdot061 \cos \odot \text{ long.} - 0''\cdot427 \sin \odot \text{ long.} + 0''\cdot487.$$

The correction required by the first term is equivalent to a reduction of $0^{s}\cdot18$ in the right ascensions of the stars employed, being the excess above Bessel's right ascensions which Professor Airy's catalogue assumes; showing that Bessel's place of the equinox is *precisely* that deducible from the Cambridge Observations. "This accuracy of coincidence," Professor Airy remarks, "is to a certain degree accidental; but I do not think that accident would extend so far as to the difference between Mr. Pond and Bessel."

The second term implies that the obliquity is less than that of the *Berliner Jahrbuch* by $0''\cdot427$.

The third term would imply that the sun moves in a *small* circle of latitude $0''\cdot487$, which increases the apparent obliquity in summer and diminishes it in winter. This result, similar to that obtained from other circles, is made the occasion of introducing the following remarks.

It is *found* that the difference of N.P.D. of a northern and southern star observed *directly* is always *less* than the same by *reflected* observation. At Cambridge a mean zenith point has been obtained from northern, zenithal, and southern stars, and subsequent single observations are corrected by comparing the zenith points deduced with the mean zenith point. All stars are thus corrected, and, by graphical interpolation, the sun and planets also; and the result is confirmatory of the method, since without it the discordance of obliquities would have amounted to five seconds. On the actual discrepancy in question, Professor Airy observes, 1st, that an error of less than $0''\cdot25$ in the correction of the circumpolar stars and the sun would entirely remove it; 2nd, that we are not justified in positively asserting the mean of direct and reflected observations to be the true difference of N.P.D. of two stars. He is disposed to attribute it to a small defect in the tables of refraction, where a trifling error in the law, the constant, or the thermometer readings, would remove it entirely. In this case the summer observations would be the more correct. But these would only require an increase of $0''\cdot06$ in the Berlin obliquity—a quantity much too small to be answered for.

IV. On the transit instrument of the Royal Observatory of St. Fernando, and the manner of using it. By Don José Sanchez Cerquero. An abstract of this paper is given in the *Monthly Notices*.

V. On the Mural Circle of the Observatory at the Cape of Good Hope. By Mr. Henderson.

Mr. Henderson has here detailed the experiments and investigations which he made for the purpose of ascertaining the cause of the anomalies which had been observed in the Cape mural circle, and of detecting the laws by which they are regulated. A similar inquiry by Mr. Sheepshanks and Professor Airy has been already published in Vol. V. of the Society's *Memoirs*.

The experiments on which Mr. Henderson grounds his investigations are a series of readings at each fifth degree of the limb of the instrument made with each of the six microscopes. The discrepancies among the readings indicate that the figure of the limb is that of an oval of small excentricity, and that the centre of motion frequently changes its position.

A change in the position of the centre of motion occasions variations in the readings of the single microscopes, which, however, disappear in taking the means of two or any other number of equidistant microscopes.

The oval figure of the instrument requires that corrections should be applied to the readings of the microscopes, in order to obtain the true values of angles which the instrument describes. Angles of 60°, however, as given by six equidistant microscopes, require no correction, they being independent of the effect of errors of division, and of the non-circularity of the instrument. The same holds true with regard to angles of 120° measured with three microscopes, and angles of 180° measured with two.

Assuming that the necessary corrections may be expressed by a series of the form

$$a \sin(\mathrm{U} + \mathrm{A}) + b \sin(2\,\mathrm{U} + \mathrm{B}) + c \sin(3\,\mathrm{U} + \mathrm{C}) + \&c.$$

a, b, c, &c., denoting constant coefficients, A, B, C, &c., constant angles, and U any division of the limb, the observed differences betwixt angles of 60° given by six microscopes, and by three and two, afford *data* for the determination of a, b, c, &c., and A, B, C, &c. Employing the method of *minimum* squares, Mr. Henderson finds the correction of the mean reading of two equidistant microscopes to be

$$5''{\cdot}523 \sin(2\,\mathrm{U} + 83^\circ\,4') + 0''{\cdot}569 \sin(4\,\mathrm{U} + 16^\circ\,46').$$

The correction of the mean reading of three equidistant microscopes is

$$= 1''{\cdot}471 \sin(3\,\mathrm{U} + 321^\circ\,8').$$

To determine the correction of the mean of six microscopes, an additional pair was required; but these not being at the Cape Observatory, Mr. Henderson has, from a consideration of the errors of angles measured with two and three microscopes, and corrected by the above formulæ, inferred that the probable error of the uncorrected mean of six microscopes is 0″·22. He also found reason to suppose, that of the 720 different sets of divisions to which observations read by the six microscopes are referred, there are, according to the laws of probability, 328 whose errors are under two tenths of a second, and one only whose error exceeds one second. This degree of accuracy, Mr. Henderson remarks, ap-

pears to equal that of the best instruments of similar construction hitherto made, and the astronomical observations confirm this estimate.

Mr. Henderson next investigates the extent of the changes in the position of the centre of motion, and the *formulæ* for representing them. It appears, that from an unsteadiness in the support of one of the pivots of the axis, the centre frequently changes its position by irregular quantities; and that, owing to the non-circular figures of the pivots, other changes in the position of the centre of motion take place while the circle revolves on its axis. Formulæ for expressing the variations occasioned by the latter cause are given, which are constant for the same positions of the circle. But neither of these causes operates in the least degree to affect the mean of the readings of equidistant microscopes; and, on the whole, Mr. Henderson is of opinion, that the accuracy of the astronomical observations made with the instrument are not impaired to any sensible extent by any of the anomalies which have been mentioned.

VI. Supplement to a paper on the Latitude and Longitude of the Cape of Good Hope. By Mr. Henderson. See *Monthly Notices*, vol. ii. p. 183, or *Memoirs*, vol. vi. p. 125.

The preceding determination, from Mr. Fallows's observations in 1829 and 1830, gave the longitude of the Cape Observatory $1^h\ 13^m\ 55^s{\cdot}8$ East. Mr. Henderson deduces $1^h\ 13^m\ 54^s{\cdot}4$ from his own observations in 1832 and 1833. The mean is $1^h\ 13^m\ 55^s{\cdot}1$, which, rejecting the $0^s{\cdot}1$, Mr. Henderson proposes to adopt, until the uncertainty is removed. The difference of longitude of Greenwich and the Cape is determined from 17 observations of the moon's first limb, and 6 observations of the second; that of Cambridge and the Cape from 16 and 8 observations of the first and second limb.

VII. Observations of the Comet of 1830, made at Ascension Island, by the late Captain Henry Foster, R.N., F.R.S., and reduced by Mr. Henderson.

VIII. Addition to a letter to Professor Airy, dated Nov. 11, 1833, on the Sun's Parallax. By Mr. Henderson. See *Monthly Notice* for April 1834, vol. iii. page 39.

Mr. Henderson has compared the Altona observations, *Ast. Nach.*, No. 240, with his own at the Cape. He finds for the values of μ (see preceding reference) from the planet's north and south limb, ·0721 and ·0507; from the mean of which he deduces $9''{\cdot}105$ for the mean horizontal parallax. The mean of all the determinations is $9''{\cdot}028$, the result being too great, as was also that of a similar method of observation in the time of Lacaille.

IX. A letter from Professor Santini to the Secretary, dated Sept. 17, 1833, containing results of computations of the perturbations of Biela's Comet.

The letter gives the results of a memoir inserted in the *Annali delle Scienze*, &c. Padua. The re-examination of the computations was undertaken to detect the difference between the results of MM. Santini and Damoiseau, as to the time of the perihelion passage. An error discovered by M. Santini in the constants of the

variation of the daily motion brought the two results much nearer; but a difference still remains between the observed times of perihelion passage in 1826 and 1832, such as would arise from a resisting medium. Towards ascertaining this point, M. Santini has recomputed the perturbations caused by *Jupiter* with the values of the mass given by Laplace and by Professor Airy. Some of the results are given in the *Monthly Notices.*

X. Stars observed with the Moon at the Royal Observatory of Cadiz, from May to December 1833. Communicated by Don Sanchez Cerquero.

XI. Occultations of ζ *Tauri*, Oct. 4, 1833,—of ε *Tauri*, March 16, 1834,—and of ν *Virginis*, April 20, 1834, observed at Saville Row. By R. Snow, Esq.

XII. Eclipse of the Sun, July 17, 1833, observed at the New Observatory of Geneva. By M. Wartmann.

The corrected times of the beginning, middle, and end of the eclipse were as follow:

	Geneva Sid. Time.
Beginning.........	1^h 3^m 52^s
Middle.............	1 53 28
End................	2 43 24

Instrument, an equatorial of Ramsden, of 25 lines' aperture; power 30. Bar. 732 millimetres; therm. + 12°·8 Reaumur; both nearly steady the whole time. The diminution of light by Nicod's photometer was almost insensible, though perfectly appreciable by the naked eye, and though two thirds of the sun's disc was eclipsed. With a power of 135, the lunar mountains were distinctly visible. M. Wartmann remarks, "*La circonférence de notre satellite parassait comme dentelée, c'est-à-dire formée d'aspérités et de dépressions.*"

XIII. Transits of the Moon with Moon-culminating Stars, observed at Cambridge Observatory in the months of April, May, and June, 1834.

ZOOLOGICAL SOCIETY.

June 10. A collection of objects of Zoology, made by Lieut. Allen, R.N., Corr. Memb. Z. S., during his late expedition up the Quorra into the interior of Africa, and presented by him to the Society, was exhibited. It was accompanied by another collection formed by the same gentleman at Fernando Po. They comprehended a previously undescribed species of *Plover*; an undescribed *Tetrodon* and a *Myletes*; specimens of *Polypterus Senegalus*, Cuv., and of a *Gymnarchus*, Ej.; and specimens of the *three-horned Chamæleon*, *Chamæleo Oweni*, Gray, and of a *Galago*, *Galago Senegalensis*, Geoff.; the two latter being from Fernando Po. They also included numerous *Insects* and *Arachnida*, both from the interior and from the island.

The bird was characterized by Mr. Gould as *Vanellus albiceps.* Between the eye and the upper mandible is situated a fleshy substance (resembling that of the *common Cock*) which hangs down at right angles with the beak; it is of an orange colour, and is narrow

in form, being one inch and a half long and half an inch wide at the base, whence it gradually tapers throughout its whole length to the tip. The spur on the shoulders is strong and sharp, and is nearly an inch in length.

The *Fishes* were characterized by Mr. Bennett, who remarked on the complete analogy borne by these species of the rivers of Western Africa to some of those of the Nile. The form of *Myletes*, Cuv., to which Lieut. Allen's fish belongs, has hitherto been obtained only in Egypt; the genus *Polypterus*, Geoff., originally observed in the Nile, seems to be limited to that river and to Senegal; the genus *Gymnarchus*, Cuv., has previously been noticed only in the Nile; and the *Tetrodon* of this collection resembles in its markings that of Egypt. The new species were characterized as *Myletes Allenii* and *Tetrodon strigosus*.

The exhibition was resumed of the new species of *Shells* collected by Mr. Cuming on the western coast of South America and among the islands of the South Pacific Ocean. Those brought on the present evening under the notice of the Society were accompanied by characters by Mr. G. B. Sowerby. They belonged to the genus PETRICOLA, and were named as follows:

PETRIC. *elliptica*, *oblonga*, *solida*, *discors*, *concinna*, *denticulata*, *rugosa*, *tenuis*, *robusta*, and *amygdalina*.

The following " Description of a new Genus of *Gasteropoda*, by W. J. Broderip, Esq., Vice President of the Geological and Zoological Societies, F.R.S., &c." was read.

SCUTELLA.

Testa Ancyliformis, intùs nitens. Apex posticus, medius, involutus. Impressiones musculares duæ, oblongo-ovatæ, laterales. Apertura magna, ovata.

Animal marinum.

This genus appears to be intermediate between *Ancylus* and *Patella*, while the aspect of the back sometimes reminds the observer of *Navicella* or *Crepidula*, Lam. Its place will most probably be among the *Cyclobranches* of Cuvier.

The two muscular impressions are situated on each side of the interior a little below the summit; while, in *Patella*, they nearly surround the internal circumference of the same part of the shell. The aperture is generally surrounded by a margin, and the *apex*, which in *Ancylus* is oblique, is central though posterior.

Mr. Cuming brought home the following species which I now proceed to describe.

SCUTELLA CRENULATA. *Scut. testâ subconicâ, cancellatâ, striis ab apice radiantibus exasperatis, albâ; intùs nitente; annulo marginali et margine crenulatis: long.* $\frac{6}{8}$, *lat.* $\frac{5}{8}$, *alt.* $\frac{4}{12}$ *poll.*

Hab. ad insulam Anäan (Chain Island).

This shell was found dead on coral sand on the beach of the island at a distance from any fresh water.

The marginal ring is very strongly developed, and the margin itself is not even; for when the shell is placed with the aperture downwards on a flat surface, it rests on the two ends, the sides of the margin forming each a low arch.

Scutella iridescens. *Scut. testá oblongo-ovatá, complanatá, minutissimè substriatá, albo et roseo guttatim tessellatá; intùs iridescente, margine interno albo, roseo maculato : long.* $\frac{3}{10}$, *lat.* $\frac{2}{10}$, *alt.* $\frac{1}{10}$ *poll.*

Hab. in Oceano Pacifico. (Grimwood's Island.)

This species was gathered by Mr. Cuming on the sands when the tide was out. There was no fresh water near, and though he obtained several individuals in the finest condition, the soft parts were gone, having evidently but lately fallen a prey to some carnivorous creature.

The shape of *Scut. iridescens* is very elegant, and the silvery iridescent nacre which lines the inside of the shell, contrasted as it is with the less brilliant but lively coloured margin, is almost dazzling. The back of the shell, which is very brittle, is mottled with white and rose colour. This disposition of its markings almost conveys the impression that the surface of the back is uneven ; but with the exception of the very minute *striæ*, which are almost imperceptible, it is smooth.

Scutella rosea. *Scut. testá subconicá, striatá, albá, lineis flammulisque roseis ornatá; intùs nitente, interdum subiridescente: long.* $\frac{1}{8}$, *lat.* $\frac{1}{12}$, *alt.* $\frac{1}{12}$ *poll.*

Obs. Varietas forsan præcedentis.

Hab. cum præcedente.

The shape and many other points in this shell differ from those of *Scut. iridescens.* Externally it is much more conical and the *striæ* which run from the *apex* to the interior margin are direct and minute, while those which are lateral are much coarser and cross the somewhat elevated white parts obliquely: in *Scut. iridescens,* the exceedingly minute *striæ* radiate evenly from the *apex.* In *Scut. rosea* we lose the brilliancy of the internal nacre which distinguishes *Scut. iridescens,* and, in some individuals, it is entirely absent. Stlil the former may only be a variety of the latter: both were found together.—W. J. B.

The *Shells* described in this communication were exhibited.

A note by Mr. G. Bennett, Corr. Memb. Z.S., was read. It gave an account of a *Pelican* now living in the grounds of Mr. Rawson at Dulwich, which wounded itself just above the breast to such an extent as to expose a spacious cavity. The bandages applied to the part were repeatedly torn off by the bird for the space of ten days, at the expiration of which the wound was healed. During the whole of the time the bird was in perfect health; eating fish and drinking as usual. The scar of the wound is still readily observable.

June 24.—A letter was read, addressed to the Secretary by Keith E. Abbott, Esq., and dated Trebizond, Dec. 10, 1833. It referred principally to a collection of objects of Zoology formed by the writer in his neighbourhood and presented by him to the Society; and contained notices of other objects which he expects to be able to procure and transmit.

It also gave some account of "the famous honey of Trebizond, which is spoken of by Xenophon in his history of the retreat of the ten thou-

sand Greeks, as having produced the effect of temporary madness or rather drunkenness on the whole of the army who ate of it, without, however, causing any serious consequences. It is supposed to be from the flowers of the *Azalea Pontica* that the *Bees* extract this honey, that plant growing in abundance in this part of the country, and its blossom emitting the most exquisite odour. The effect which it has on those who eat it is, as I have myself witnessed, precisely that which Xenophon describes: when taken in a small quantity it causes violent head-ache and vomiting, and the unhappy individual who has swallowed it resembles as much as possible a tipsy man; a larger dose will completely deprive him of all sense and power of moving for some hours afterwards." A portion of the honey accompanied the letter, and was exhibited.

The other objects presented by Mr. Keith Abbott were also exhibited.

At the request of the Chairman, Mr. Gould brought the *Birds* severally under the notice of the Meeting. Their principal interest rested on the assistance afforded by a collection formed in such a locality towards the determination of the geographical limits of certain species. Those among the *Birds* of Europe which are found in India also would, it is reasonable to anticipate, occur in the intermediate locality of Trebizond; but there are, among the Trebizond *Birds*, various European species which do not, as far as is yet known, occur in India, and the existence of which in so eastern a range is consequently interesting. A list of them, with remarks by Mr. Gould as to their localities, is given in the Proceedings of the Society.

XLIII. *Intelligence and Miscellaneous Articles.*

PREPARATION OF OSMIUM AND IRIDIUM. BY M. PERSOZ.

THE method of separating these metals depends, 1st, upon the action of crude platina upon mixtures of carbonate of potash or soda with sulphur, which produce sulphurets of iron, osmium and iridium, and a silicate of these bases, and which collect in the form of scoriæ on the surface of the mass in fusion; 2ndly, upon the action of oxygen at a high temperature on the sulphuret of osmium, and from which results a blue volatile compound, described by Berzelius.

The process of sulphuration for acting upon metals has already been employed with so much success by MM. Berthier and Wohler, that I had every reason to hope it might be applied to the extraction of osmium and iridium, the alloy of which is one of the most refractory known.

A perfect mixture is to be made of one part of the ore of platina, after the action of aqua regia, two parts of carbonate of soda, and three parts of sulphur: this mixture is to be gradually projected into an earthen crucible previously made red hot, and when the whole has been put in, the crucible is to be covered, and kept white hot for a few minutes. When cold the bottom of the crucible will be found to contain a button formed of small crystals having the appearance of pyrites; these are the sulphurets of all the metals which the ore con-

tained, and some of them are combined with sulphuret of sodium. This button is covered with a layer of pure sulphuret of sodium, the middle of which contains a small quantity of the crystals of sulphuret of osmium; lastly, the surface of the fused mass is the crust of silicate of a light brown colour.

The scoriæ being separated from the fused mass of sulphurets, it is to be put into water; this dissolves the alkaline sulphuret, the double sulphuret of platina, if any should exist in it, and the sulphuret of sodium combined with the sulphuret of osmium and iridium; there remain suspended in the liquid, the sulphurets of iron, osmium, and iridium, which are to be suffered to subside; by repeatedly washing and subsiding all the metallic sulphurets, are separated from the fragments of crucibles and scoriæ.

The separated sulphurets are to be treated with hot dilute muriatic acid, which dissolves the iron with the evolution of sulphuretted hydrogen gas. As soon as the action is over, the whole is to be thrown on a filter, which retains the sulphurets of osmium and iridium: these when thoroughly washed have the appearance of plumbago. In order to separate the osmium from the iridium, a mixture is made of one part of these sulphurets and three parts of sulphate of mercury: this is to be put into an earthen retort, with a tube adapted for the disengagement of gas. The retort is to be placed in a common furnace, and gradually to be made intensely hot. As soon as the retort becomes nearly red hot, a great quantity of sulphurous acid is evolved, and the heat increasing, vapours arise, which condense on the sides of the adopter into a dense indigo coloured liquid. When the evolution of gas has ceased, the apparatus is allowed to cool, and oxide of iridium is found in the retort. To reduce it to the metallic state it is to be heated in a porcelain tube and hydrogen gas passed over it: when cooled in this gas, the metal is obtained, resembling spongy platina in appearance. It readily inflames hydrogen gas.

Sometimes the iridium is not quite free from osmium; in this case it is to be fused with potash in a silver crucible: osmiate of potash is formed, which dissolves in water, and a little iridium is also taken up. The whole is to be filtered: the oxide of iridium remaining on the paper is to be well washed, and then dissolved in muriatic acid. To this solution, when concentrated, muriate of ammonia is to be added, this occasions the precipitation of a double black chloride, which after washing and calcining yields pure iridium. Much osmium is found in the neck and adopter of the retort, but in the former it is in the state of oxide combined with mercury, while the latter contains the blue substance already mentioned, and consisting of osmium, oxygen, and sulphur.

To separate the osmium from the mercury, it is sufficient to introduce it into a glass tube, slightly inclined, through which a current of hydrogen is passed, while the tube is slightly heated, the mercury is volatilized, and the pure osmium remains.

The osmium of the blue compound may be separated by zinc, or still better by treating it merely with water, which converts it into another compound of a brown colour and insoluble in water. When

washed and dried it may be reduced by hydrogen in the manner above mentioned: in this operation water and sulphuretted hydrogen are produced.—*Ann. de Chim. et de Phys.*, tom. lv. p. 210.

PURIFICATION OF CARBONATE OF SODA. BY M. GAY LUSSAC.

This salt is commonly purified by repeated crystallization, but it retains so large a quantity of interposed mother liquor that many operations are required to entirely free it from other substances, and after all but a small quantity is obtained. The following process having appeared to me extremely advantageous, I think it may be useful to make it known.

This process is analogous to that which is followed in France for purifying nitre. It is as follows:—Take the crystals of carbonate of soda, such as are met with in shops; having washed them, make a saturated hot solution; when this is set to cool, stir constantly with a rod or spatula to disturb the crystallization and to obtain small crystals resembling sand: the cooling may be accelerated by placing the vessel containing the saline solution in cold water. It sometimes happens that when very much cooled the solution does not crystallize, and that it suddenly becomes solidified. This is the moment to stir very rapidly, to prevent the conglomeration of the crystals. This delay in the crystallization may be prevented by throwing a few crystals into the solution at the moment when it begins to be supersaturated.

Having obtained the crystals, put them into a funnel, in the neck of which place a little tow or cotton to retain them. At first let them drain, then wash them with small quantities of distilled water, waiting till the preceding washing has run through. Test from time to time the washings with nitrate of silver, the washing being previously saturated with pure nitric acid: the purification of the salt is complete when the liquid remains transparent. By this process, and in the first operation, the greater part of the carbonate of soda employed may be obtained in a perfectly pure state. The mother liquor and the washing may be evaporated and treated in the same manner. The same mode of purifying may be used with advantage for many other salts. Its efficacy is founded upon the extreme facility with which water runs through and well washes sandy crystals, such as are obtained by disturbing the crystallization.—*Ann. de Chim. et de Phys.*, tom. lv. p. 221.

FOSSIL WAX OF MOLDAVIA. BY M. MAGNUS.

This substance is evidently a mixture of several different matters. It is indeed true that this is not immediately seen, for though it has sometimes the fibrous structure of amianthus, and is sometimes conchoidal in its fracture, it yet appears to be homogeneous; if, however, one of the small leaves of which it is composed be carefully examined, small deep coloured points are visible. When the fossil wax is boiled with even absolute æther or alcohol, but a very small quantity is dissolved, whilst the greater part of the remaining portion has an eaten appearance, which shows that the mass is composed of two substances,

one of which is soluble and the other insoluble in alcohol. They are, however, so intimately mixed, that it is impossible to separate them by mechanical means. Oil of turpentine, at a high temperature, dissolves fossil wax completely.

It melts at 177° Fahr. without suffering any change. Its fusing point is therefore higher than that of bee's wax, which is 144° Fahr.; the fossil wax does not lose its greenish brown colour nor its peculiar empyreumatic odour by melting.

In order to explain the formation of this wax, it appeared to me interesting to know if it contained any azote. I burnt it with oxide of copper according to Liebig's method. 0·200 gr. yielded 0·2755 of water and 0·6205 of carbonic acid, which show that it is composed of

Carbon	85·75
Hydrogen	15·15
	101·00

The excess was owing to the volatilization of a portion of the wax without decomposition, which took place in spite of every precaution. It contains therefore neither oxygen nor azote, and its composition approaches that of olefiant gas.—*Ibid.* p. 218.

CAUTION TO EXPERIMENTERS WITH THE ELECTRICAL KITE. BY MR. STURGEON.

On Friday last, about half-past two in the afternoon, clouds began to form in various quarters of the heavens in rapid succession, from mere specks or streaks to immense groups, with every appearance of being highly electrical.

I repaired to the Artillery Barrack grounds with an electric kite, and in a very short time got it afloat, letting out string through the hands from a coil or clue which was thrown on the ground. When about a hundred yards of the string had been let out, a tremendous discharge took place, which gave me such a blow in the chest and legs that I became completely stunned, let go the string, and consequently the kite soon fell.

The accident was owing entirely to my own neglect, and could not possibly have happened had I taken the following precaution.

Let all the string intended to be employed be first taken off the reel or coil, and stretched on the ground. Let now the insulating cord, riband, glass, or whatever is used for this purpose, be attached to the kite-string and fastened to a peg, tree, or anything intended to hold the kite during the time it is up. Next fasten the kite to the other end of the string, and let it ascend from the hand.

This is the manner in which I usually proceed when heavy clouds are hovering about, and ought always to be attended to, although I neglected it on this occasion. The experimenter by this means is completely out of danger; and he may easily ascertain if the string be highly charged by going to the other end, because of the brushes of light, and noise attending them.

I find it convenient to have a sliding copper wire on the silken cord, which can be moved, by means of a long glass rod, to any re-

quired distance from the wired string, the other end being stuck fast in the ground. If the electric fire strikes two inches over the dry silken cord, (and it will sometimes strike a yard,) it would not be safe to approach it; and no man could hold the string when it strikes over one inch of the silk, or, which is the same thing, through the air.

After the electrical state of the string has been ascertained, the wire may be slided away from it as far as possible (the silk ought never to be less than two yards long). The other end is then to be taken out of the ground and attached to the apparatus for experiment. The wire is again slided up to the wired string, and left there during the time the experiments are carrying on.

The only method of getting the kite down during an intense electrization of the string, with safety to the experimenter, is to unfasten the silken cord from its hold and let all go: the kite falls. I have frequently been annoyed, whilst holding the kite-string during hot hazy days when no cloud was visible, by a rapid succession of discharges, from which I had no other means of escape than by quitting the string and letting the kite fall. The same thing sometimes happens in cold dense fogs in the winter. I have experienced these rattling or tremulous shocks when the kite has not been more than 30 yards from the ground, and the wired string at the same time touching it. Hence great quantities of the fluid must necessarily pass into the ground directly through the wire, in addition to that which produced the shocks.

The publication of these particulars may possibly prevent some inexperienced electrician from receiving a death-blow from his kite-string.

Young persons who are fond of kite-flying should also be cautious not to have their kites up during thunder storms, as it is possible that a wet string may transmit a violent discharge, from which a serious accident might occur.

Artillery Place, Woolwich, July 23, 1834. W. STURGEON.

ON SOME REMARKABLE CRYSTALS OF SNOW. BY W. THOMPSON, ESQ., V.P. BELFAST NAT. HIST. SOC.

On the 22nd of March 1833, when travelling outside a stage-coach from London to Shrewsbury, and near to Daventry, the day being up to this time mild and calm, (the weather for some weeks previously had been excessively cold, with prevalent easterly and north-easterly winds,) snow of the loose flaky kind, common to the climate, began to fall, but mingled with it there appeared beautifully delicate lamellar crystals, of uniform transparency, having a spherical nucleus, from which sprang 6 and 12 radii most exquisitely formed, all the rays on each species being equal, and not in a single instance deviating from the regularity of geometrical proportion, as has on some occasions been observed. By far the greater number of these were of the former species, "having 6 points radiating from a centre." The figures 20 and 94 in the plates of snow crystals in Scoresby's "Arctic Regions" represent both these crystals, the lines

exhibited as extending from the centre of the latter not having been however visible to the naked eye. These stellate crystals, varying from $1\frac{1}{2}$ to 2 lines in extreme diameter, continued to fall for nearly half an hour, and as they retained their form for a considerable time, proved highly interesting and attractive. I much regretted that I was so circumstanced as to be unable to ascertain the state of the barometer and thermometer; the wind, however, was northerly with a point of east. Snow continued to fall until the coach reached Shrewsbury; and in passing through Wales the next day, which was remarkably fine, I observed that it had fallen there in every direction. Arriving in Ireland on the 24th, I remarked that the snow had also extended to that country, the Wicklow and Dublin mountains being covered with it.

In anticipation of it being thought singular that I have not before recorded the fall of these ice-crystals, it may be stated that I delayed in the hope that some one better circumstanced for precise investigation had noticed the occurrence, and would have written on the subject of it. In expectation of this I have looked over the various British periodicals since published, and nothing having appeared concerning it, I presume it to be better that even the present imperfect account should be recorded than none at all.

Wm. Thompson.

OBITUARY:—PROFESSOR HARDING.

We have to record the death of Professor Harding of the University Göttingen, an eminent astronomer, whose name will go down to posterity with the important discovery of the planet Juno, which it was his good fortune to make in 1804. He was descended from a highly respectable English Catholic family. One of his ancestors left England on account of his religion, and settled in Germany, where the family afterwards became Protestants. He was born at Lauenburg, the principal town of the then Hanoverian, now Danish duchy of Lauenburg, and was originally intended for the church; but after his academical studies, he became tutor to the son of the celebrated astronomer Schröter, and this circumstance led him to the study of practical astronomy, to which he afterwards exclusively devoted his whole life. After having been several years astronomical assistant to Schröter, he accepted in 1805 a Professorship of Astronomy at Göttingen, which he retained till his death.

Professor Harding was a most active and industrious practical astronomer, whose observations have, in no small degree, enlarged our knowledge of the heavenly bodies. He rendered a very important service to astronomy by compiling accurate maps of those parts of the heavens in which planets may be expected to appear. The perseverance and careful attention with which he mastered the heavens during several years in the prosecution of this work, were rewarded by the brilliant discovery above alluded to. He was a very amiable man, whose loss is much deplored by his numerous friends and colleagues in the university. The grief at the loss of his daughter, an only child, 14 years old, who died last year, terminated his days on the 31st of August last, at the age of about 70 years.

Meteorological Observations made by Mr. THOMPSON at the Gardenof the Horticultural Society at Chiswick, near London, and by Mr. VEALL, at Boston.

Days of Month. 1834.	Barometer.			Thermometer.			Wind.		Rain.	
	London.		Boston $8\frac{1}{2}$ A.M.	London.		Bost. $8\frac{1}{2}$ A.M.	Lond.	Bost.	Lond.	Bost.
	Max.	Min.		Max.	Min.					
Aug. 1	29·858	29·851	29·66	82	63	68	W.	calm	0·10	...
2	29·906	29·873	29·31	76	51	68	NE.	NE.	...	...
3	29·891	29·856	29·30	77	55	64	N.	calm	...	...
4	29·912	29·910	29·23	72	58	68	S.	calm	...	...
● 5	29·863	29·850	29·16	71	58	67	SW.	calm	...	...
6	29·916	29·852	29·15	74	55	67·5	SW.	W.	·58	...
7	29·964	29·882	29·27	68	61	65	S.	W.	·50	0·05
8	29·935	29·771	29·05	74	50	68	W.	W.	·20	...
9	30·150	30·074	29·43	72	49	66	W.	NW.	...	...
10	30·148	30·071	29·50	80	50	68	S.	calm	...	...
☽ 11	30·050	30·032	29·36	80	51	67	W.	calm	...	...
12	30·172	30·013	29·43	84	53	68	S.	calm	...	...
13	30·089	29·962	29·19	85	56	72	SW.	S.	...	...
14	30·092	30·057	29·45	71	53	66	NE.	NE.	...	...
15	30·139	30·085	29·50	73	60	66	NE.	E.	...	...
16	30·187	30·163	29·11	76	55	65·5	E.	E.	...	...
17	30·154	30·027	29·57	77	54	67	SE.	calm	...	...
18	29·954	29·920	29·36	77	55	64	NE.	calm	...	...
○ 19	29·934	29·886	29·37	73	58	63	NE.	calm	...	...
20	29·793	29·685	29·17	76	54	61	W.	calm	...	·13
21	29·747	29·671	29·12	74	49	62·5	SW.	calm	...	·07
22	29·825	29·764	29·16	72	46	59	SW.	calm	...	...
23	29·928	29·894	29·35	71	40	57	SW.	calm	...	·02
24	29·791	29·634	29·26	65	43	58	S.	calm	·23	...
25	29·725	29·706	29·10	67	43	56·5	SW.	calm	...	...
26	29·820	29·764	29·33	70	41	56	SW.	calm	·11	...
☾ 27	29·843	29·784	29·30	71	41	52	NW.	calm	...	·09
28	29·927	29·859	29·35	67	54	56	SW.	W.	·02	...
29	29·737	29·624	29·16	68	53	63	S.	S.	·01	·92
30	29·683	29·644	29·07	72	55	61	S.	W.	·92	·07
31	29.944	29·723	29·10	70	53	61·5	SW.	W.	·06	·04
	30·187	29·624	29·28	85	40	63·6			2·73	1·39

Remarks.

London.—August 1. Foggy: fine. 2. Cloudy: fine. 3. Very fine. 4. Foggy: very fine. 5. Cloudy and fine. 6. Heavy showers. 7. Showery: heavy rain at night. 8. Rain. 9. Sultry. 10—22. Very fine. 23. Fine: slight showers. 24. Fine: heavy showers at night. 25. Showery. 26. Slight showers: very fine. 27. Heavy dew: fine. 28. Fine but cool: rain at night. 29. Cloudy, with slight showers. 30. Fine: very heavy rain at night. 31. Cloudy and fine.

Boston.—August 1. Cloudy. 2—4. Fine. 5. Cloudy. 6. Fine: rain P.M. 7, 8. Cloudy. 9—18. Fine. 19. Cloudy: rain A.M. 20. Cloudy: rain P.M. 21. Fine. 22. Fine: rain P.M. 23, 24. Fine. 25. Cloudy. 26. Fine: rain P.M. 27. Fine. 28. Fine: rain P.M. 29. Cloudy: rain P.M. 30. Cloudy. 31. Fine: rain early A.M.

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XLIV. *Experiments on Light**. *By* H. F. Talbot, *Esq., M.P., F.R.S.*

§ 1. *Microscopic Appearances with Polarized Light.*

Among the very numerous attempts which have been made of late years to improve the microscope, I am not aware that it has yet been proposed to illuminate the objects with polarized light.

But as such an idea is sufficiently simple and obvious, it is possible that some experiments of this kind may have been published, although I am not acquainted with them. I have lately made this branch of optics a subject of inquiry, and I have found it so rich in beautiful results as entirely to surpass my expectations.

As little else is requisite to repeat the experiments which I am about to mention than the possession of a good microscope, I think that in describing them I shall render a service to that numerous class of inquirers into nature, who are desirous of witnessing some of the most brilliant of optical phænomena without the embarrassment of having to manage any large or complicated apparatus. And it cannot be without interest for the physiologist and natural historian to present him with a method of microscopic inquiry, which exhibits objects in so peculiar a manner that nothing resembling it can be produced by any arrangements of the ordinary kind.

In order to view objects by polarized light, I place upon

* A paper read to the Royal Society, July 1834.

the stage of the microscope a plate of tourmaline, through which the light of the large concave mirror is transmitted before it reaches the object lens. Another plate of tourmaline is placed between the eyeglass and the eye; and this plate is capable of being turned round in its own plane, so that the light always traverses both the tourmalines perpendicularly. The mirror being adjusted so as to reflect the daylight into the body of the microscope, and no object being as yet placed on the stage, it follows, as indeed is obvious to all who are conversant with this branch of optics, that if the two tourmalines are placed in a similar position, the light freely traverses them both: but if that which is next the eye is turned round 90°, the observer can perceive nothing, except when the light of the sun is used, which causes a small remnant of the light to become visible. Except in this case, however, the field of view is quite dark in this position of the tourmalines, and partially bright in the opposite position. It is only partially, and not entirely bright, because even the best tourmalines have a considerable tinge of brown or green colour, which greatly disturbs and disfigures the colours of all bodies which are viewed through them.

In consequence of this defect I found it advisable to abandon the use of tourmalines, and to adopt, instead, an arrangement of single-image calcareous spar, the invention of which is due to Mr. Nicol. As I have examined the theory of this instrument in another place*, I shall not revert to it at present further than to say that the effect it produces is entirely similar to that of the tourmaline, but that it possesses over that mineral the precious advantage of perfect whiteness and transparency.

This instrument has nearly the form of a four-sided prism with a rhomboidal base, and it is conveniently placed in the axis of a small brass tube, which is screwed on as close above the eyeglass as possible, and forms a prolongation of the tube of the microscope. A similar tube is screwed beneath the stage of the microscope to transmit and at the same time polarize the light of the mirror.

This arrangement is found to answer extremely well, for when the two polarizing instruments are placed in a similar position, the field of view is bright and perfectly white; but when the one next the eye is turned round 90°, the field of view is completely dark. It is in the latter situation that the microscope is generally to be used.

Having fitted up a microscope in the manner described, let us take for the object of examination a hair. In order to make

* See Lond. and Edinb. Phil. Mag., vol. iv. p. 289.

it perfectly transparent, and to prevent the diffraction of light, it should be immersed in oil or varnish and pressed between two plates of glass. The field of view having been darkened as much as possible in the manner before described, the hair is placed upon the stage of the microscope, and it is then seen to be beautifully luminous. As no other light reaches the eye than that which traverses the hair, every minute circumstance in its interior structure is plainly distinguished. It is almost needless to say that this effect is owing to the depolarizing power of the hair. Many organic substances of animal and vegetable origin appear luminous in a similar way, while others, on the contrary, are inert, and have no such action upon light.

Fragments of coarsely powdered sugar and of various kinds of salts appear more or less bright, and mottled with various colours; but common salt is an exception, as it remains nearly or altogether dark, at least when it is employed in a state of purity.

But when instead of examining amorphous fragments of these salts we view them regularly and carefully crystallized, the phænomena become much more interesting. Sulphate of copper offers a very excellent example. This salt, which is of a fine blue colour when viewed in considerable thicknesses, is white and transparent when it is extremely thin, and its crystals can be procured so small as to be quite destitute of perceptible coloration. A drop of it was placed upon a warm piece of glass and suffered to evaporate gradually. The crystals shooting from the edge of the drop into the interior of the liquid had a long and narrow rectangular form, with a slanting extremity, which may be compared in shape to the straight edge of a chisel. Seen by common light these crystals offer nothing peculiar, but on the darkened field of the microscope they are luminous and splendidly coloured, the colour depending upon the thickness of the crystal, and being the same in all points of its surface, except upon the little inclined plane which forms its extremity. But upon this oblique portion are seen three or four distinct bands of colour parallel to the edge and offering to the eye a visible scale or measure of the rapid diminution of thickness in that part. The observed succession of colours in one experiment was the following. Yellow, brown, purple, blue, sky blue, straw yellow, yellow, reddish purple, blue, sea green, green, greenish yellow, pink, green, pink, blueish green, pink.

Taking, again, for the subject of experiment the sulphate of copper, we may make it crystallize in quite another manner, by not employing any heat, but simply touching the drop

with an exceedingly small portion of nitric æther. This causes in a short time a deposition of minute crystals in the form of rhomboids, exquisitely perfect in shape and transparency, and of every variety of size.

When these miniature crystals are placed on the stage of the microscope, the field of view remaining dark as before, we see a most interesting phænomenon; for as every crystal differs from the rest in thickness, it displays in consequence a different tint, and the field of view appears scattered with the most brilliant assemblage of rubies, topazes, emeralds, and other highly coloured gems, affording one of the most pleasing sights that can be imagined. The darkness of the ground upon which they display themselves greatly enhances the effect. Each crystal is uniform in colour over all its surface, but if the plate of glass upon which they lie is turned round in its own plane, the colour of each crystal is seen to change and gradually to assume the complementary tint. Many other salts may be substituted for the sulphate of copper in this experiment, and each of them offers some peculiarity, worthy of attention, but difficult to describe. Some salts, however, crystallize in such thin plates that they have not sufficient depolarizing power to become visible upon the dark ground of the microscope. For instance, the little crystals of sulphate of potash, precipitated by æther, appear only faintly visible. In these circumstances a contrivance may be employed to render evident their action upon light. It must be obvious that if a thin uniform plate of mica is viewed with the microscope, it will appear coloured (the tint depending on the thickness it may happen to have), and its appearance will be everywhere alike, in other words it will produce a *coloured field of view.* Now if such a plate of mica is laid beneath the crystals, or beneath the glass which supports them, these crystals, although incapable of producing any colour themselves, are yet frequently able to alter the colour which the mica produces; for instance, if the mica has produced a blue, they will, perhaps, alter it to purple, and thus will have the appearance of purple crystals lying on a blue ground.

Such are the appearances when we examine crystals perfect in form and structure. But the opposite extreme of the phænomenon is also worth considering, namely, when the crystallization is as irregular as possible, which is the case, for instance, when a few drops of a saline solution are evaporated by rapid heat. The salt chosen should of course be of a kind which acts powerfully upon light. When different portions of such a crystalline film are examined successively, the alternations of colour succeed each other so rapidly and in such a

capricious manner, as to baffle description, and with care it is often possible to detect five or six different shades of colour upon some of the very smallest of the crystalline fragments. These miniature phænomena are often highly curious. Upon one occasion a solution of some salt was precipitated by alcohol in very minute crystals. When the alcohol began to evaporate, it caused currents in the liquid, which carried the crystals across the field of view, and at the same time caused them to revolve round their centres.

The crystals, which were highly luminous in one position, when their axes were in the proper direction for depolarizing the light, became entirely dark in the opposite position, thus, as they rapidly moved onwards, appearing by turns luminous and obscure, and resembling in miniature the coruscations of a firefly. It was impossible to view this without admiring the infinite perfection of nature, that such almost imperceptible atoms should be found to have a regular structure capable of acting upon light in the same manner as the largest masses, and that the element of light itself should obey in such trivial particulars the same laws which regulate its course throughout the universe.

In what has preceded I have always supposed the polarizing eyepiece in such a situation as to darken the field of view. If we now turn it round 90°, the field of view becomes bright. But we are now presented with a new set of appearances, which though not so striking as the last, still have considerable interest. Even in this position the apparatus often imparts a strong tinge of colour to crystals which are quite white by ordinary light; and this is sometimes so intense as to produce complete opacity. As this fact appears to me to be new, and is particularly striking, I will mention that I have most frequently observed it with the salts of copper and nickel, but it also occurs with other salts.

The singularity of this appearance is enhanced by turning round the polarizing eyepiece; for when the field of view becomes dark, the crystal becomes white and luminous, and when the field becomes bright, the crystal darkens and becomes entirely opaque. I have not yet satisfied myself concerning the cause of this remarkable phænomenon, which is not in accordance with the received theory; but it probably arises from something peculiar in the chemical nature of these crystals, which causes them to deviate from the ordinary law of action with respect to the differently coloured rays.

A circumstance which frequently accompanies these experiments is too remarkable to be passed over in silence.

The crystals which form in the liquid under examination are

often observed in a state of rapid growth, increasing in all their dimensions, while they accurately retain the same geometrical figure. With the polarizing microscope they are seen to change their tint as they increase in size, and thus, for instance, it often happens that a crystal which possessed a red colour at the moment of the first observation, if viewed again after the lapse of a single minute, may have turned to green; and again, after the lapse of another minute, it will, perhaps, have become blue.

I have said enough to show that I regard the polarizing microscope as an instrument of great promise, and although I have not had leisure to undertake any very extensive series of experiments with it, yet I will add two instances by way of illustration of the manner in which it develops the structure of transparent bodies, and I will choose for my examples the sulphate of potash and the acetate of copper, in both of which I am able to confirm the phænomena announced by preceding observers.

Sulphate of Potash.—In the year1819, Sir David Brewster discovered that the bipyramidal sulphate of potash, which had been always considered as a simple crystal, was, on the contrary, a compound of three others, very curiously jointed together, in such a way as to produce a six-sided pyramid.

This he ascertained by making a section of the crystal and viewing a plate of it by polarized light. (See Edinburgh Philosophical Journal, vol. i. p. 6.)

But by proper management I find the sulphate of potash may be made to crystallize in thin hexagonal plates, and thus their structure may be examined without the trouble of cutting them. When a perfect hexagon of this kind is viewed in the polarizing microscope, it is seen to be divided into six coloured triangles, bounded by the six lines which go from the centre of the hexagon to its angles. There are, however, only three different colours, because each triangle has always the same colour with the one opposite to it. It is worth remark that the six boundary lines are visible even by common light, resembling small veins or cracks in the crystal.

Acetate of Copper.—In this salt Sir David Brewster detected the beautiful property of dichroism. (See Phil. Trans. for 1819, or Brewster's Optics, p. 252.)

By cutting a plate of it from a large crystal, and examining it by polarized light, he found that it changed its tint from blue to green according to the position in which the crystal was held. Now this curious observation receives the most ample confirmation from the polarizing microscope. I found the following a convenient method of performing the experi-

ment. A drop of saturated solution of sulphate of copper was precipitated by ammonia. The precipitate was again dissolved by more of the alkali, taking care to add no more than was requisite for that purpose. Some strong aceticacid was then added, and the whole stirred together with a glass rod, upon a plate of glass, which was then laid upon the stage of the microscope. In about half a minute spontaneous crystallization took place of a very beautiful salt, which I presume to be the acetate of ammonia and copper, in rhomboids of a deep blue colour, and very perfect in shape, insomuch that among many hundreds of them the microscope detected scarcely any that had any flaw or defect of transparency. In shape these crystals are not unlike those of common sulphate of copper, but they are distinguished from them by a decisive character, namely, their deep blue colour, even when of the very smallest dimensions: for the crystals of sulphate of copper, when of this degree of minuteness, are perfectly white. Now, when the incident light is polarized, it causes a singular change in these crystals; for one half of them become green, the other half acquiring a purer blue than before. But on turning round the plate of glass that supports them, the blue ones are seen to change their tint to green, while the green in their turn become blue: thus exhibiting the dichroism of which Sir David Brewster was the first discoverer; and exhibiting it, let it be observed, in crystals too small to be seen with the naked eye, and without the observer's having the trouble of cutting and polishing their surfaces.

Potash may be substituted for ammonia in this experiment, but soda will not answer the purpose.

§ 2. *On Photometry.*

The subjects of optical inquiry are so diversified and distinct that I have found it convenient to arrange the experiments I am relating under several separate heads, which have perhaps, little or no connexion with each other. Those mentioned in the last section were made lately, while, on the contrary, those in the present section, relating to photometry, were made nine years ago, and should have been published at that time, except that I wished to render them more worthy of publication by following up the many other trains of experiment which they suggested. An article in a foreign journal having recalled my attention to this subject, I will proceed to give a short account of the view I took of it formerly, and which I have seen no reason to alter since.

Photometry, or the measurement of the intensity of light, has been supposed to be liable to peculiar uncertainty. At

least no instrument that has been proposed has met with general approval and adoption. I am persuaded, nevertheless, that light is capable of accurate measurement, and in various ways; and that the difficulties which stand in the way of obtaining a convenient and accurate instrument for photometrical purposes will ultimately be overcome.

To begin with the most simple considerations which may serve to guide us in reasoning on this subject, I will select the experiment well known to every one of whirling round a glowing coal, which from its rapid motion conveys to the eye the impression of a continuous fiery circle. The question deserves consideration whether the eye receives from this circular ring exactly the same quantity of light which it received from the much smaller surface of the coal at rest. There can be no doubt, as it seems, that such must be the case; for if the luminous circle sent more rays to the eye, it would likewise send more in every other direction, and thus the apartment would become more illuminated than before, which is not the fact. If, then, the total quantity of light remains the same, it follows that its apparent intensity must have diminished exactly in the same proportion as its apparent area has been enlarged. For the sake of more accuracy if we confine our reasoning to a very small portion of the luminous body, the enlarged area which it seems to occupy is evidently proportional to the circumference of the circle it describes; therefore the preceding argument alleges that the intensity of light diminishes as the circumference increases. But in the same proportion in which the circumference increases, the time during which the coal is found in any particular point of the circle diminishes.

The rapidity of the rotation does not affect the argument. For instance, if the rotation is in a vertical plane, the time during which the coal occupies the summit of the circle, compared with the time of one whole revolution, necessarily diminishes when the circle grows larger; for the time of its being so situated bears the same proportion to the whole time, that the diameter of the moving body (which is constant) bears to the whole circumference through which it moves. Since, then, these two things—the intensityof light and the time of the body's remaining in any given part of the circle—are each inversely proportional to the circumference of the circle it describes, it follows that they must be directly proportional to each other; that is to say, that a regularly intermittent luminary whose observations are too frequent and too transitory for the eye to perceive, loses so much of its apparent brightness from this cause, as is indicated by the pro-

portion between the whole time of observation, and the time during which it disappears. This seems a very simple law, and therefore likely to be true; but such an experiment is much too inaccurate to be relied upon to establish it. But as it suggests a very important idea, namely, that *time* may be employed to measure the intensity of *light*, it becomes desirable to establish it on solid grounds. And this is easily accomplished; we have only to take a white circle, with one of its sectors painted black, and make it revolve rapidly. It will appear, as every one knows, of a uniform gray tint, without any variation from the centre to the circumference; and yet, if we look first at a point near its centre, and afterwards at a point near its circumference, a much greater portion of the white surface will pass beneath the eye in the latter case than in the former. But a greater portion of the black surface likewise passes, and causes a compensation. In every point of the circle the white and black parts meet the eye during the same proportion of time, and therefore the tint is uniform throughout.

This experiment, though very simple and well known, seems yet well calculated to detect any error in the assumed law, if it should deviate in the least from the truth; for such a deviation would cause a perceptible want of uniformity in some parts of the circle which the eye would not fail to discover. I need hardly observe that it would be illogical to assert *à priori* the existence of this law of optics, however simple and natural it may appear, unless we were perfectly well acquainted with the circumstances which accompany the action of light upon the retina, which is very far from being the case. Its proof can rest upon experiment alone, and by that it appears to be most satisfactorily established.

Assuming white paper, at least for the present, as our standard of whiteness, and applying to the revolving circle black sectors of different angles, we obtain, when they are set in motion, various gray tints, the obscuration in which is proportional to the angle of the sector; and therefore, if we make an assortment of numerous shades of gray paper, and ascertain by direct comparison with the revolving wheel that in any particular experiment its tint matches one of the papers, we have only to mark upon the paper the angle of the sector employed, and we may ever after employ the paper with confidence in questions of a similar kind. According to this notation perfect whiteness would be represented by 0, and perfect blackness by 360.

This method may be applied to determine experimentally the intensity of a polarized ray. We may view through a

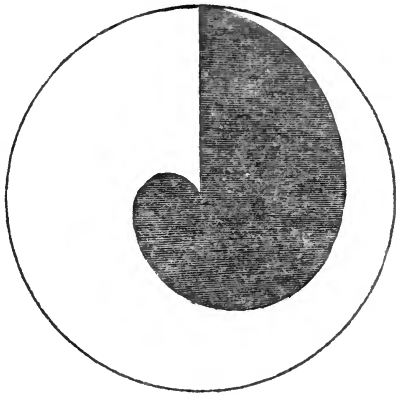
piece of doubly refracting spar a small white disk placed upon a black ground. We may cause the two images partially to overlap each other, and thus have a portion common to both. This portion will be perfectly white; but the remaining portions of each image will evidently have exactly one half of white light. Their tint will therefore agree with that of the revolving wheel when one half of it is painted black.

But by combining two pieces of spar, images may be formed of variable intensity, according to the position of the axes of the spar, and they may either be compared with the gray papers previously graduated, or directly with the indications of the revolving wheel. Having thus found the proportion of white light in the images for any given position of this polarizing instrument, it may afterwards itself become eminently employed as a photometer. The colours hitherto used were white and black, but in order to verify the results more completely, any two other colours may be chosen, and the mixed tint arising from their union may be employed instead of the gray.

Having thus considered the appearance of a whirling disk containing one black sector, I next made the following variation in the apparatus. I described a great number of concentric circles, and drew a common diameter to them, and marked off upon each circumference a number of degrees (reckoned from the diameter) which diminished regularly from 360° to 0 as the circles grow larger; *i. e.* in the smallest circle I took nearly all the circumference, in the middle circle I took half the circumference, and in the outer circle the arc was reduced to nothing. The extremities of all the arcs being joined together, formed a line belonging to the class of spirals, but having only one convolution, and bounded by the common diameter.

(See figure 1.)—This spiral figure was painted black, the rest of the circle remaining white, and the wheel was then made to revolve. The result was a gray surface varying gradually and regularly, in concentric shades, from perfect blackness in the centre to perfect whiteness in the circumference. I then chose a number of coloured papers and cut them into circles of the same size, each of which had this spiral traced upon it, which I afterwards cut out from the rest of the circle. By this means, being all of a size, they could be made to replace each other in a great variety of ways; for instance, the blue spiral was put on the yellow circle, and the yellow spiral on the blue circle. When in motion, I obtained in each case a surface varying gradually from perfect blue to perfect yellow, the central part being blue in the first case, and yellow in the

latter. But the most curious part of this experiment was to observe the neutral tint, through which the colour passed at a certain point. When this neutral tint offered any singularity that made it desirable to examine it more particularly, it was easy, by measuring its distance from the centre, to compute how much of each colour entered into its composition; for instance, if the diameter of the wheel, formed of yellow and blue, was six inches, and the tint in question was two inches from the centre (supposed blue), it contained two thirds of blue and one third of yellow, and therefore I had only to make a blue circle, and place on it a yellow sector of 120°, to obtain infallibly the same tint over all its surface.



It may be objected to these experiments with revolving disks, that they afford no assistance when the light of lamps, &c., is the subject of comparison. But in that case, the principle remaining the same, the application of it must be varied, and we must observe the luminary through a revolving wheel whose spokes are gradually tapering towards the centre. If there are twelve spokes each of which forms a sector of five degrees, their sum is 60 degrees, and the proportion of this to the whole circle, viz. one sixth, is the degree of obscuration produced by the wheel when rapidly revolving upon all objects that are seen through it.

If the wheel has eighteen spokes, each of which is a sector of 10°, their sum is 180°, and therefore, a lamp seen through them loses one half of its light, and every other luminous object the same constant proportion. But since it is requisite to have the power of producing a *variable* obscuration, this may be accomplished by having a second wheel similar to the first, and placed close to it upon the same axis, so that they may be capable of being fastened together in any required position. If they are placed so that their spokes correspond, the two wheels will, of course, only produce the same effect that either of them would separately; but if the spokes of one correspond to the intervals of the other, they will prevent any

light from passing. Whence it is plain that intermediate positions may be found which give any precise degree of obscuration that may be desired.

The next step I took in pursuing this inquiry was to reverse the first experiment, of a luminous body in motion, by viewing its image reflected from a revolving mirror. For instance, the image of the sun was made to describe a portion of a great circle of the heavens by causing the mirror to revolve round an axis properly situated. The moving image thus produced a band of light half a degree broad, and if the preceding reasoning is true, the intensity of light in the central part of this band is diminished in the proportion of the sun's diameter to 360°, that is, of 1 to 720. The band is therefore, in its central part, 720 times fainter than the sun's image at rest, (reflected from the same mirror,) and towards its borders it is gradually fainter. But as the light will still be too bright for comparison with terrestrial flames, it must be diminished further by a second operation of the same kind. And this may be done in various ways. The most simple seems to be to employ a screen having an aperture through which a small portion of the band only can be seen, for instance, half a degree of its length, and then treating this separated portion as a new luminary. By help of a second revolving mirror it may be again weakened 720 times, so that the effect on the eye is then diminished a number of times expressed by the square of 720, or about half a million, great care being, however, necessary in this second experiment as to the position of the observer's eye*. For a photometrical comparison of objects which are exceedingly unequal in

* As this seems liable to error, except when the second mirror is so distant from the screen that all its parts may be considered equidistant from it, the following method may be preferable, though it involves some mathematical calculation. It has been already remarked, that the sides of the luminous band or zone are fainter than its central part; because the image of the sun's disk takes a shorter time to traverse any point in that zone the more distant it is from the central line. This want of uniformity requires that we should determine what is the *average* brightness of the zone; and this resolves itself into the mathematical problem, "What is the average value of all the chords of a circle, supposing an infinite number of them to be drawn infinitely near to each other (but equidistant)? And the answer given to this question by the integral calculus is, that the average line is equal in length to the quadrantal arc of the circle. This arc bears to the diameter the ratio of 785 to 1000 nearly, therefore the average brightness of the zone is only $\frac{785}{1000}$ of the brightness of its central part.

Now, if by help of a second revolving mirror, we cause the image of this zone to move in a direction perpendicular to its length, it will be expanded

brightness, I think I have here suggested a method that is founded upon sound principles.

But here let me be permitted to leave for a moment the immediate subject of this paper for the purpose of offering a suggestion relating to the measurement of quantities in various other branches of natural philosophy. I think the same method which I have here advanced as leading to a correct numerical estimation of intense light, is also applicable to the measurement of very intense heat. It is well known what discrepancy of opinion exists as to the proportion of ordinary temperatures to those which are very exalted (such, for instance, as that of melted platina). Might we not measure this heat with an ordinary thermometer, provided we so contrive the experiment that its exposure to the heat shall be intermittent, and shall only last for an extremely small portion of the whole time? To illustrate this idea, let us suppose a red-hot cannon ball suspended near the edge of a rapidly revolving wheel. If a thermometer were fixed on the circumference of the wheel, it would be only exposed to the influence of the high temperature during a small part (let us suppose, for example's sake, a hundredth part) of its revolution. If, therefore, it was observed to mount five degrees, we might fairly argue that it would have sustained an increase of temperature of 500°, if it had remained stationary at the point nearest to the ball.

It is true, that in this form the experiment would be very inaccurate, owing to the cooling power of the atmosphere on the revolving thermometer and other causes, therefore I only mention it for the sake of illustration; or both the heated body and the thermometer might be at rest, and a rotatory screen might intervene between them, having an aperture of given dimension, during the passage of which the thermometer would be affected by the heat, but not at other times.

If there is any truth in the preceding argument, as I trust there is, it offers a method (and perhaps the only possible one of subjecting to numerical comparison some qualities of

into a surface uniform in light, which will have $\frac{1}{(720)^2} \times \frac{785}{1000}$, that is, about $\frac{1}{660,000}$ of the brightness of the sun; that is, speaking accurately, not of the sun himself, but of his image reflected successively from both mirrors when at rest. It is, however, to be observed, that the velocities of the two mirrors must not be equal, nor in any simple ratio to one another, otherwise their effect would be different from what is here contemplated.

bodies which have never, I believe, been even attempted to be measured, such as the intensity of odours, &c.; for this principle seems to have a general application. We may always find means of dividing the experiment into minute intervals of time, and we may cause that quality of the body which we wish to estimate the intensity of to act upon our senses or upon our instruments, only during a certain number of those intervals, but regularly and rapidly recurring in a stated order.

XLV. *Experimental Researches in Electricity.—Seventh Series.* *By* Michael Faraday, *D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c.*

[Continued from p. 264.]

¶ vii. *On the definite Nature and Extent of Electro-chemical Decomposition.*

783. In the third series of these Researches, after proving the identity of electricities derived from different sources, and showing, by actual measurement, the extraordinary quantity of electricity evolved by a very feeble voltaic arrangement (371. 376.), I announced a law, derived from experiment, which seemed to me of the utmost importance to the science of electricity in general, and that branch of it denominated electro-chemistry in particular. The law was expressed thus: *The chemical power of a current of electricity is in direct proportion to the absolute quantity of electricity which passes* (377.).

784. In the further progress of the successive investigations, I have had frequent occasion to refer to the same law, occasionally in circumstances offering powerful corroboration of its truth (456. 504. 505.); and the present series already supplies numerous new cases in which it holds good (704. 722. 726. 732.). It is now my object to consider this great principle more closely, and to develope some of the consequences to which it leads. That the evidence for it may be the more distinct and applicable, I shall quote cases of decomposition subject to as few interferences from secondary results as possible, effected upon bodies very simple, yet very definite in their nature.

785. In the first place, I consider the law as so fully established with respect to the decomposition of *water*, and under so many circumstances which might be supposed, if anything could, to exert an influence over it, that I may be excused

entering into further detail respecting that substance, or even summing up the results here (732.). I refer, therefore, to the whole of the subdivision of this series of Researches which contains the account of the *volta-electrometer*.

786. In the next place, I also consider the law as established with respect to *muriatic acid* by the experiments and reasoning already advanced, when speaking of that substance, in the subdivision respecting primary and secondary results (758, &c.).

787. I consider the law as established also with regard to *hydriodic acid* by the experiments and considerations already advanced in the preceding division of this series of Researches (767. 768.).

788. Without speaking with the same confidence, yet from the experiments described, and many others not described, relating to hydro-fluoric, hydro-cyanic, ferro-cyanic, and sulpho-cyanic acids (770. 771. 772.), and from the close analogy which holds between these bodies and the hydro-acids of chlorine, iodine, bromine, &c., I consider these also as coming under subjection to the law, and assisting to prove its truth.

789. In the preceding cases, except the first, the water is believed to be inactive; but to avoid any ambiguity arising from its presence, I sought for substances from which it should be absent altogether; and, taking advantage of the law of conduction already developed (380. &c.), soon found abundance, amongst which *protochloride of tin* was first subjected to decomposition in the following manner. A piece of platina wire had one extremity coiled up into a small knob, and having been carefully weighed, was sealed hermetically into a piece of bottle-glass tube, so that the knob should be at the bottom of the tube within (fig. 13.). The tube was suspended by a piece of platina wire, so that the heat of a spirit-lamp could be applied to it. Recently fused proto-chloride of tin was introduced in sufficient quantity to occupy, when melted, about one half of the tube; the wire of the tube was connected with a volta-electrometer (711.), which was itself connected with the negative end of a voltaic battery; and a platina wire connected with the positive end of the same battery was dipped into the fused chloride in the tube; being, however, so bent, that it could not by any shake of the hand or apparatus touch the negative electrode at the bottom of the vessel. The whole arrangement is delineated fig. 14.

790. Under these circumstances the chloride of tin was decomposed: the chlorine evolved at the positive electrode formed bichloride of tin (779.), which passed away in fumes,

and the tin evolved at the negative electrode combined with the platina, forming an alloy, fusible at the temperature to which the tube was subjected, and therefore never occasioning metallic communication entirely through the decomposing chloride. When the experiment had been continued so long as to yield a reasonable quantity of gas in the volta-electrometer, the battery connexion was broken, the positive electrode removed, and the tube and remaining chloride allowed to cool. When cold, the tube was broken open, the rest of the chloride and the glass being easily separable from the platina wire and its button of alloy. The latter when washed was then reweighed, and the increase gave the weight of the tin reduced.

791. I will give the particular results of one experiment, in illustration of the mode adopted in this and others, the results of which I shall have occasion to quote. The negative electrode weighed at first 20 grains; after the experiment it, with its button of alloy, weighed 23·2 grains. The tin evolved by the electric current at the *cathode* weighed, therefore, 3·2 grains. The quantity of oxygen and hydrogen collected in the volta-electrometer = 3·85 cubic inches. As 100 cubic inches of oxygen and hydrogen, in the proportions to form water, may be considered as weighing 12·92 grains, the 3·85 cubic inches would weigh 0·49742 of a grain; that being, therefore, the weight of water decomposed by the same electric current as was able to decompose such weight of protochloride of tin as could yield 3·2 grains of metal. Now 0·49742 : 3·2 :: 9 the equivalent of water is to 57·9, which should therefore be the equivalent of tin, if the experiment had been made without error, and if the electro-chemical decomposition *is in this case also definite.* In some chemical works 58 is given as the chemical equivalent of tin, in others 57·9. Both are so near to the result of the experiment, and the experiment itself is so subject to slight causes of variation (as from the absorption of gas in the volta-electrometer (716.), &c.), that the numbers leave little doubt of the applicability of the *law of definite action* in this and all similar cases of electro-decomposition.

792. It is not often I have obtained an accordance in numbers so near as that I have just quoted. Four experiments were made on the protochloride of tin, the quantities of gas evolved in the volta-electrometer being from 2·05 to 10·29 cubic inches. The average of the four experiments gave 58·53 as the electro-chemical equivalent for tin.

793. The chloride remaining after the experiment, was pure protochloride of tin; and no one can doubt for a mo-

ment that the equivalent of chlorine had been evolved at the *anode*, and having formed bichloride of tin as a secondary result, had passed away.

794. *Chloride of lead* was experimented upon in a manner exactly similar, except that a change was made in the nature of the positive electrode; for as the chlorine evolved at the *anode* forms no perchloride of lead, but acts directly upon the platina, if that metal be used, it produces a solution of chloride of platina in the chloride of lead; in consequence of which a portion of platina can pass to the *cathode*, and will produce a vitiated result. I therefore sought for, and found in plumbago, another substance, which could be used safely as the positive electrode in such bodies, as chlorides, iodides, &c. The chlorine or iodine does not act upon it, but is evolved in the free state; and the plumbago has no reaction, under the circumstances, upon the fused chloride or iodide in which it is plunged. Even if a few particles of plumbago should separate by the heat or the mechanical action of the evolved gas, they can do no harm in the chloride.

795. The mean of three experiments gave the number of 100·85 as the equivalent for lead. The chemical equivalent is 103·5. The deficiency in my experiments, I attribute to the solution of part of the gas (716.) in the volta-electrometer; but the results leave no doubt on my mind that both the lead and the chlorine are, in this case, evolved in *definite quantities* by the action of a given quantity of electricity (814. &c.).

796. *Chloride of Antimony.*—It was in endeavouring to obtain the electro-chemical equivalent of antimony from the chloride that I found reasons for the statement I have made respecting the presence of water in it in an earlier part of these Researches (690. 693. &c.).

797. I endeavoured to experiment upon the *oxide of lead* obtained by fusion and ignition of the nitrate in a platina crucible, but found great difficulty, from the high temperature required for perfect fusion, and the powerful fluxing qualities of the substance. Green glass tubes repeatedly failed. I at last fused the oxide in a small porcelain crucible, heated fully in a charcoal fire; and as it was essential that the evolution of the lead at the *cathode* should take place beneath the surface, the negative electrode was guarded by a green glass tube, fused around it in such a manner as to expose only the knob of platina at the lower end (fig. 15.), so that it could be plunged beneath the surface, and thus exclude contact of air or oxygen with the lead reduced there. A platina wire was employed for the positive electrode, that metal not being sub-

ject to any action from the oxygen evolved against it. The arrangement is given fig. 16.

798. In an experiment of this kind the equivalent for the lead came out 93·17, which is very much too small. This, I believe, was because of the small interval between the positive and negative electrodes in the oxide of lead, so that it was not unlikely that some of the froth and bubbles formed by the oxygen at the *anode* should occasionally even touch the lead reduced at the *cathode*, and re-oxidize it. When I endeavoured to correct this by having more litharge, the greater heat required to keep it all fluid caused a quicker action on the crucible, which was soon eaten through, and the experiment stopped.

799. In one experiment of this kind I used borate of lead (408. 673.). It evolves lead, under the influence of the electric current, at the *anode*, and oxygen at the *cathode*; and as the boracic acid is not either directly (408.) or incidentally decomposed during the operation, I expected a result dependent on the oxide of lead. The borate is not so violent a flux as the oxide, but it requires a higher temperature to make it quite liquid; and if not very hot, the bubbles of oxygen cling to the positive electrode, and retard the transfer of electricity. The number for lead came out 101·29, which is so near to 103·5 as to show that the action of the current had been definite.

800. *Oxide of Bismuth.*—I found this substance required too high a temperature, and acted too powerfully as a flux, to allow of any experiment being made on it, without the application of more time and care than I could give at present.

801. The ordinary *protoxide of antimony*, which consists of one proportional of metal and one and a half of oxygen, was subjected to the action of the electric current in a green glass tube (789.), surrounded by a jacket of platina foil, and heated in a charcoal fire. The decomposition began and proceeded very well at first, apparently indicating, according to the general law (679. 697.), that this substance was one containing such elements and in such proportions as made it amenable to the power of the electric current. This effect I have already given reasons for supposing may be due to the presence of a true protoxide, consisting of single proportionals (696. 693.). The action soon diminished, and finally ceased, because of the formation of a higher oxide of the metal at the positive electrode. This compound, which was probably the peroxide, being infusible and insoluble in the protoxide, formed a crystalline crust around the positive electrode;

and thus insulating it, prevented the transmission of the electricity. Whether if it had been fusible and still immiscible it would have decomposed, is doubtful, because of its departure from the required composition (697.). It was a very natural secondary product at the positive electrode (779.). On opening the tube it was found that a little antimony had been separated at the negative electrode; but the quantity was too small to allow of any quantitative result being obtained.

802. *Iodide of Lead.*—This substance can be experimented with in tubes heated by a spirit-lamp (789.); but I obtained no good results from it, whether I used positive electrodes of platina or plumbago. In two experiments the numbers for the lead came out only 75·46 and 73·45, instead of 103·5. This I attribute to the formation of a periodide at the positive electrode, which dissolving in the mass of liquid iodide, came in contact with the lead evolved at the negative electrode, and dissolved part of it, becoming itself again protiodide. Such a periodide does exist; and it is very rarely that the iodide of lead formed by precipitation, and well washed, can be fused without evolving much iodine, from the presence of this percompound; nor does crystallization from its hot aqueous solution free it from this substance. Even when a little of the protiodide and iodine are merely rubbed together in a mortar, a portion of the periodide is formed. And though it is decomposed by being fused and heated to dull redness for a few minutes, and the whole reduced to protiodide, yet that is not at all opposed to the possibility, that a little of that which is formed in great excess of iodine at the *anode*, should be carried by the rapid currents in the liquid into contact with the *cathode*.

803. This view of the results was strengthened by a third experiment, where the space between the electrodes was increased to one third of an inch; for now the interfering effects were much diminished, and the number of the lead came out 89·04; and it was fully confirmed by the results obtained in the cases of transfer to be immediately described (818.).

The experiments on iodide of lead, therefore, offer no exception to the *general law* under consideration, but, on the contrary, may, from general considerations, be admitted as included in it.

804. *Protiodide of Tin.*—This substance, when fused (402.), conducts and is decomposed by the electric current, tin is evolved at the *anode*, and periodide of tin as a secondary result (779. 790.) at the *cathode*. The temperature required for its fusion is too high to allow of the production of any results fit for weighing.

805. *Iodide of potassium* was subjected to electrolytic action in a tube, fig. 13. (789.). The negative electrode was a globule of lead, and I hoped in this way to retain the potassium, and obtain results that could be weighed and compared with the volta-electrometer indication; but the difficulties dependent upon the high temperature required, the action upon the glass, the fusibility of the platina induced by the presence of the lead, and other circumstances, prevented me from obtaining such results. The iodide was decomposed with the evolution of iodine at the *anode*, and of potassium at the *cathode*, as in former cases.

806. In some of these experiments several substances were placed in succession, and decomposed simultaneously by the same electric current; thus, protochloride of tin, chloride of lead, and water, were thus acted on at once. It is needless to say that the results were comparable, the tin, lead, chlorine, oxygen, and hydrogen evolved being definite in quantity and electro-chemical equivalents to each other.

807. Let us turn to another kind of proof of the *definite chemical action of electricity.* If any circumstances could be supposed to exert an influence over the quantity of the matters evolved during electrolytic action, one would expect them to be present when electrodes of different substances, and possessing very different chemical affinities for the evolving bodies, were used. Platina has no power in dilute sulphuric acid of combining with the oxygen at the *anode*, though the latter be evolved in the nascent state against it. Copper, on the other hand, immediately unites to the oxygen, as the electric current sets it free from the hydrogen; and zinc is not only able to combine with it, but can, without any help from the electricity, abstract it directly from the water, at the same time setting torrents of hydrogen free. Yet in cases where these three substances were used as the positive electrodes in three similar portions of the same dilute sulphuric acid, specific gravity 1·336, precisely the same quantity of water was decomposed by the electric current, and precisely the same quantity of hydrogen set free at the *cathodes* of the three solutions.

808. The experiment was made thus. Portions of the dilute sulphuric acid were put into three basins. Three volta-electrometer tubes, of the form figg. 5, 7. were filled with the same acid, and one inverted in each basin (707.). A zinc plate, connected with the positive end of a voltaic battery, was dipped into the first basin, forming the positive electrode there, the hydrogen, which was abundantly evolved from it by the direct action of the acid, being allowed to escape. A

copper plate, which dipped into the acid of the second basin, was connected with the negative electrode of the *first* basin; and a platina plate, which dipped into the acid of the third basin, was connected with the negative electrode of the *second* basin. The negative electrode of the third basin was connected with a volta-electrometer (711.), and that with the negative end of the voltaic battery.

809. Immediately that the circuit was complete, the *electro-chemical action* commenced in all the vessels. The hydrogen still rose in, apparently, undiminished quantities from the positive zinc electrode in the first basin. No oxygen was evolved at the positive copper electrode in the second basin, but a sulphate of copper was formed there; whilst in the third basin the positive platina electrode evolved pure oxygen gas, and was itself unaffected. But in *all* the basins the hydrogen liberated at the *negative* platina electrodes was the *same in quantity*, and the same with the volume of hydrogen evolved in the volta-electrometer, showing that in all the vessels the current had decomposed an equal quantity of water. In this trying case, therefore, the *chemical action of electricity* proved to be *perfectly definite*.

810. A similar experiment was made with muriatic acid diluted with its bulk of water. The three positive electrodes were zinc, silver, and platina; the first being able to separate and combine with the chlorine *without* the aid of the current; the second combining with the chlorine only after the current had set it free; and the third rejecting almost the whole of it. The three negative electrodes were, as before, platina plates fixed within glass tubes. In this experiment, as in the former, the quantity of hydrogen evolved at the *cathodes* was the same for all, and the same as the hydrogen evolved in the volta-electrometer. I have already given my reasons for believing that in these experiments it is the muriatic acid which is directly decomposed by the electricity (764.); and the results prove that the quantities so decomposed are *perfectly definite* and proportionate to the quantity of electricity which has passed.

811. In this experiment the chloride of silver formed in the second basin retarded the passage of the current of electricity, by virtue of the law of conduction before described (394.), so that it had to be cleaned off four or five times during the course of the experiment; but this caused no difference between the results of that vessel and the others.

812. Charcoal was used as the positive electrode in both sulphuric and muriatic acids (808. 810.); but this change produced no variation of the results. A zinc positive elec-

trode, in sulphate of soda or solution of common salt, gave the same constancy of operation.

813. Experiments of a similar kind were then made with bodies altogether in a different state, *i. e.* with *fused* chlorides, iodides, &c. I have already described an experiment with fused chloride of silver, in which the electrodes were of metallic silver, the one rendered negative becoming increased and lengthened by the addition of metal, whilst the other was dissolved and eaten away by its abstraction. This experiment was repeated, two weighed pieces of silver wire being used as the electrodes, and a volta-electrometer included in the circuit. Great care was taken to withdraw the negative electrode so regularly and steadily that the crystals of reduced silver should not form a *metallic* communication beneath the surface of the fused chloride. On concluding the experiment the positive electrode was re-weighed, and its loss ascertained. The mixture of chloride of silver, and metal, withdrawn in successive portions at the negative electrode, was digested in solution of ammonia, to remove the chloride, and the metallic silver remaining also weighed: it was the reduction at the *cathode*, and exactly equalled the solution at the *anode*; and each portion was as nearly as possible the equivalent to the water decomposed in the volta-electrometer.

814. The infusible condition of the silver at the temperature used, and the length and ramifying character of its crystals, render the above experiment difficult to perform, and uncertain in its results. I therefore wrought with a chloride of lead, using a green glass tube, formed as in fig. 17. A weighed platina wire was fused into the bottom of a small tube, as before described (789.). The tube was then bent to an angle, at about half an inch distance from the closed end; and the part between the angle and the extremity being softened, was forced upward, as in the figure, so as to form a bridge, or rather separation, producing two little depressions or basins *a*, *b*, within the tube. This arrangement was suspended by a platina wire, as before, so that the heat of a spirit-lamp could be applied to it, such inclination being given to it as would allow all air to escape during the fusion of the chloride of lead. A positive electrode was then provided, by binding up the end of a platina wire into a knob, and fusing about twenty grains of metallic lead on to it, in a small closed tube of glass, which was afterwards broken away. Being so furnished, the wire with its knob was weighed, and the weight recorded.

815. Chloride of lead was now introduced into the tube, and carefully fused. The leaded electrode was also intro-

duced; after which the metal, at its extremity, soon melted. In this state of things the tube was filled up to *c* with melted chloride of lead; the end of the electrode to be rendered negative was in the basin *b*, and the electrode of melted lead was retained in the basin *a*, and, by connexion with the proper conducting wire of a voltaic battery, was rendered positive. A volta-electrometer was included in the circuit.

816. Immediately upon the completion of the communication with the voltaic battery, the current passed, and decomposition proceeded. No chlorine was evolved at the positive electrode; but as the fused chloride was transparent, a button of alloy could be observed gradually forming and increasing in size at *b*, whilst the lead at *a* could also be seen gradually to diminish. After a time, the experiment was stopped; the tube allowed to cool, and broken open; the wires, with their buttons, cleaned and weighed; and their change in weight compared with the indication of the *volta-electrometer*.

817. In this experiment the positive electrode had lost just as much lead as the negative one had gained (795.), and the loss or gain was very nearly the equivalent of the water decomposed in the volta-electrometer, giving for lead the number 101·5. It is therefore evident, in this instance, that causing a *strong affinity*, or *no affinity*, for the substance evolved at the *anode*, to be active during the experiment (807.), produces no variation in the definite action of the electric current.

818. A similar experiment was then made with iodide of lead, and in this manner all confusion from the formation of a periodide avoided (803.). No iodine was evolved during the whole action, and finally the loss of lead at the *anode* was the same as the gain at the *cathode*, the equivalent number, by comparison with the result in the volta-electrometer, being 103·5.

819. Then protochloride of tin was subjected to the electric current in the same manner, using, of course, a tin positive electrode. No bichloride of tin was now formed (779. 790.). On examining the two electrodes, the positive had lost precisely as much as the negative had gained; and by comparison with the volta-electrometer, the number for tin came out 59.

820. It is quite necessary in these and similar experiments to examine the interior of the bulbs of alloy at the ends of the conducting wires; for occasionally, and especially with those which have been positive, they are cavernous, and contain portions of the chloride or iodide used, which must be removed before the final weight is ascertained. This is more usually the case with lead than tin.

821. All these facts combine into, I think, an irresistible

mass of evidence, proving the truth of the important proposition which I at first laid down, namely, *that the chemical power of a current of electricity is in direct proportion to the absolute quantity of electricity which passes* (377. 783.). They prove, too, that this is not merely true with one substance, as water, but generally with all electrolytic bodies; and, further, that the results obtained with any *one substance* do not merely agree amongst themselves, but also with those obtained from *other substances*, the whole combining together into *one series of definite electro-chemical actions* (505.). I do not mean to say that no exceptions will appear: perhaps some may arise, especially amongst substances existing only by weak affinity; but I do not expect that any will seriously disturb the result announced. If, in the well considered, well examined, and, I may surely say, well ascertained doctrines of the definite nature of ordinary chemical affinity, such exceptions occur, as they do in abundance, yet, without being allowed to disturb our minds as to the general conclusion, they ought also to be allowed if they should present themselves at this, the opening of a new view of electro-chemical action; not being held up as obstructions to those who may be engaged in rendering that view more and more perfect, but laid aside for a while, in hopes that their perfect and consistent explanation will finally appear.

[To be continued.]

XLVI. *Account of the new Observatory for Magnetic Observations at Göttingen*.*

WE are indebted to the munificence of Government for a new institution, devoted to an important part of natural philosophy, namely, an Observatory, erected for magnetic observations and measurements. Although the building was commenced last autumn, and the interior arrangements so far advanced at the beginning of this year that daily observations could be made in it ever since that period, we have forborne giving an account of it in this publication, as we wished to add at the same time some of the results of the observations. The magnetical apparatuses, constructed on new principles, and placed, in 1832, in our astronomical observatory, have been fully described in a former Number†, from which it will be seen that very exact results may be obtained by their use.

* From the *Göttingische gelehrte Anzeigen* for August 1834.

† A translation of Prof. Gauss's memoir in which these apparatus are described, will be found in our report of the proceedings of the Royal Society for Feb. 14, 1833: Lond. and Edinb. Phil. Mag. vol. ii. p. 291.—EDIT.

But to render this exactness complete, the apparatuses were required to be made on a larger scale; and, to procure for the results a perfect freedom from extraneous influences, it was also necessary to erect a building entirely free from iron.

The magnetic observatory stands in an open space about a hundred paces west of the astronomical one, and forms an oblong square, placed due north and south, of 32 Paris feet in length, by 15 in breadth, with two projections on the longer sides. Of these the western forms the entrance, and also serves for certain observations as an extension of the space within; while the eastern, which is perfectly parted off from that principal apartment, is appropriated to the watchman of the observatory. Everything in the building usually made of iron, such as locks, hinges, window linings, nails, &c., is made here of copper. Drafts are as much as possible prevented. The height of the apartment is 10 feet. The magnetic apparatus being essentially the same as that alluded to above, we shall here confine ourselves to merely pointing out the differences. The magnetic bar is made of *Uslar* cast steel, which is particularly fit for magnetic observations; and it is frequently replaced by another, both being very nearly of the same size, viz. 610 millimetres in length, 37 in breadth, and 10 in thickness, having a weight of nearly four pounds. The mirror is 75 millimetres broad and 50 high. The suspension of the rod from the ceiling is effected by means of a 200-fold untwisted silk thread 7 feet long; but the circle of torsion is not, as before, at the upper, but at the lower, end of the thread, and so connected with the little skiff bearing the rod as to turn with it. Silk threads, as has been observed in Mr. Gauss's treatise, *Intensitas Vis magneticæ terrestris*, (p. 19,) have the great advantage over metallic ones that their power of torsion is but small: in the present thread it is only the nine hundredth part of the horizontal power of direction of the magnetic rod; whilst the twisting power of a metallic thread of the same strength for bearing would be about ten times as great. On the other hand, silk threads, especially if their power of supporting does not much exceed the weight attached to them, have the inconvenience of considerably stretching during the first weeks, or by a great increase of weight. This inconvenience has been here removed by an ingenious suspension apparatus, suggested by Professor Weber, fastened to the ceiling, and by which the thread may be wound up as much as necessary, without changing its place; while at the same time, this apparatus itself can be shifted about the ceiling, if, in the course of time, a removal should become necessary, by

a variation in the magnetic declination. The theodolite has hitherto been standing on a very solid wooden pedestal on a stone foundation; and from its position may be seen, through the northern window, one of the steeples of the town, the azimuth of which has been exactly determined. A delicate vertical line on the opposite northern wall is the only mark by which the unmoved position of the theodolite is rectified. A scale, 4 feet long, divided into millimetres, is used in common; but for some observations it is changed for one 2 metres long. The value of a division of the scale is $21''\cdot3$. For night observations the scale has been hitherto strongly lighted with wax candles; but in future Argand lamps will be used instead. One of the principal uses of the apparatus now is to determine with the utmost exactness the magnetic declination, and its variations in different hours of the day, months, and years. The noting takes place twice every day at fixed hours, viz. at 8 in the morning and 1 P.M., with which times, in a regular course of daily variations, the smallest and the greatest declination, at least during the first months of the year, pretty nearly coincide. This manner of noting exactly at the same hours, (instead of waiting for the minimum and maximum of every day,) has been preferred for important reasons, which need not be entered upon in this place. The noting was begun as early as the 1st of January, but because at first the thread of suspension, being too weak, required, in consequence of its gradual stretching, to be frequently wound up, which produced rather considerable variations in the zero point of torsion,—at first not sufficiently noticed,—the [observations of the] first two months and a half have been excluded. The mean values of the western declination of the magnetic needle have since been as follows:

	8 o'Clock A.M.	1 o'Clock P.M.
March, second half	18° 38′ 16″·0	18° 46′ 40″·4
April..................	36 6 ·9	47 3 ·8
May	36 28 ·2	47 15 ·4
June..................	37 40 ·7	47 59 ·5
July	37 57 ·5	48 19 ·0

Moreover, on certain days of the year the variations of the declination are observed uninterruptedly for 44 hours, at short intervals. For this purpose those days were chosen which several years ago were fixed on by M. de Humboldt, and on which by agreement similar observations are noted

with Gambey's apparatus in other places, some of which are very remote from each other. Till now, such observations have been made here three times, viz. on the 20—21 March, 4—5 May, and 21—22 June; and the persons engaged in it were, besides Mr. Gauss, the Professors Weber and Ulrich, Doctors Weber, Goldschmidt, and Listing, and Messrs. Sartorius, Deahna, and Wilh. Gauss. The object of these observations is, partly to obtain gradually a fuller acquaintance with the regular course [of the phænomena], partly to discover the nature of the very great anomalies occurring at times, especially with auroræ boreales, by contemporaneous observations in different places. The notings here were made in March, at intervals of 20 minutes, and even of half that time; in May, every 10 and even 5 minutes; and in June, always at intervals of 5 minutes. Some anomalies were observed, particularly a few very great ones in the night of the 20 to 21 March; very considerable and numerous ones in the night of the 4—5 May; and some decided, though not great ones, on the 21 June, whilst the 22nd of that month was exceedingly regular. We have as yet heard nothing of the results of corresponding observations instituted at the suggestion of M. de Humboldt, except of those made at Berlin on the 21—22 March, which were, however, only noted from hour to hour, and could, therefore, furnish no satisfactory results. Still they contained an indication of the anomalies noted and pursued at Göttingen. On the other hand corresponding observations were made very fully on the 4—5 May, by Mr. Sartorius, in a country house in Bavaria, a few miles south of Meiningen, with an apparatus on the same principle as that of our observatory, although smaller; which produced a most wonderful agreement with the great anomalies observed here, as to time, magnitude and variations, so that in their noting they appear almost copies of each other in all the mixed figures produced by those anomalies. An equally beautiful result was obtained by corresponding observations made in Berlin on the 21—22 by Professor Encke, assisted by Messrs. Poggendorff, Mädler, and Wolfers, for the first time with an apparatus like ours, although likewise smaller. Also there were no other anomalies but those observed here; but these were almost exact copies; and the same was shown by the observations made at that time at Francfort by Mr. Sartorius. These results may already be considered as a beautiful effect of preconcerted observations, since they clearly show that both the small and great anomalies in the magnetic needle, which are observed sometimes at very short intervals, must be ascribed to causes by no means local, but powerful ones and operating at great

distances; a fact which had indeed been noticed before in cases of very great irregularities in connexion with auroræ boreales. It is therefore to be expected that, as the participation in these preconcerted observations with this both convenient and exact apparatus increases (for which there are already the best prospects), we shall obtain very remarkable and comprehensive solutions of these very singular and till now inexplicable phænomena.

Similar observations have frequently been made here at other times also, from which sometimes very striking anomalies have resulted. Thus, for instance, on the 14th January, in the evening between 8 and 9 o'clock, the declination decreased, within a quarter of an hour, by 13 minutes, with the greatest regularity, and then gradually returned to its former position. Such observations, however, can produce no further results, since, without a preconcerted arrangement, corresponding observations are very seldom to be expected.

From time to time also the experiments relative to the determination of the absolute intensity of terrestrial magnetism will be repeated at the observatory; but as they require various preparations to perform them with the greatest exactness, the first could not be made till the month of July. Three determinations with different bars gave the following results:

July 17th	1·7743
—— 20th	1·7740
—— 21st	1·7761

as the value of the horizontal power by which, as in the earlier determinations with smaller bars, the second of time, the millimetre, and the milligramme form the foundation as units.

The new apparatus in the magnetical observatory has also been adapted for making electro-magnetic observations and measurements, in a similar manner as in the astronomical observatory. The suspended magnetic rod is surrounded by a multiplier consisting of 200 circumvolutions, the construction of which permitted the use of uncovered wire: the length of the wire is 1100 feet. With the help of a very simply constructed commutator, the observer may, without taking his eye from the telescope, reverse the direction of, or entirely interrupt, the galvanic current.

Nor can we omit mentioning on this occasion an apparatus on a grand and hitherto unique scale, connected with the apparatuses just described, and which we owe to our Professor Weber. This gentleman had during the last year conducted a double line of wires from the cabinet of natural philosophy over the houses of the town to the astronomical observatory. These are now continued to the magnetical observatory, thus

forming a chain by which the galvanic current, including the multipliers attached to each end of the chain, has to run through a length of wire of nearly 9000 feet. The wire is mostly copper, of the thickness known in trade as number 3, and of which a length of one metre weighs 8 grammes. The wire of the multiplier in the magnetical observatory is of plated copper number 14, of which one gramme gives 2·6 metres. This arrangement is likely to produce the most interesting results. It is wonderful how a single pair of plates placed at the other extremity immediately impart to the magnetic rod a motion equal to considerably more than a thousand divisions of the scale. But what is still more remarkable is, that a pair of plates, of perhaps no more than one inch in diameter, will produce, with the application of mere spring or distilled water, an effect not much less than that produced by a very large pair of plates with the application of a strong acid. This circumstance, however, is after all not surprising, as it only tends to confirm the beautiful theory first established by Ohm. On the other hand, by adding to the number of the pairs of plates the effect increases, and almost in exact proportion to it. The facility and certainty by which, through the means of the commutator, the current and the movement of the needle dependent on it are commanded, suggested the idea of trying telegraphic experiments with the apparatus, which have perfectly succeeded with whole words, and even short phrases. Nor is there a doubt that by this means telegraphic communications might be formed between towns many miles apart; but this is, of course, not the place where we could enter on a further development of this subject.

XLVII. *On the Magneto-electric Spark and Shock, and on a peculiar Condition of Electric and Magneto-electric Induction. By* MICHAEL FARADAY, *Esq., D.C.L., F.R.S., &c.*

To Richard Phillips, Esq., F.R.S., &c.

My dear Sir,

IF you think well of the following facts and reasoning, you will, perhaps, favour them with a place in the Philosophical Magazine.

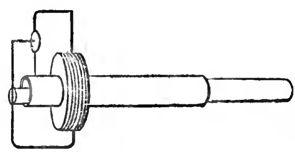
When I first obtained the magneto-electric spark*, it was by the use of a secondary magnet, rendered for the time active by a principal one; and this has always, as far as I am aware, been the general arrangement. My principal was an electro-magnet; Nobili's was, I believe, an ordinary magnet; others

* Philosophical Transactions, 1832, p. 132. [See also Phil. Mag. and Annals, N.S. vol. xi. p. 401, &c.—EDIT.]

have used the natural magnet, but in all cases the secondary magnet was a piece of soft iron.

The spark is never the electricity of the principal, or even of the secondary magnet. The power in the first induces a corresponding power in the second, and that induces a motion of the electricity in the wire round the latter, which electricity produces the spark. It seemed to me, however, no difficult matter to dispense with the secondary or temporary magnet, and thus approach a step nearer to the original one; and this was easily accomplished in the following manner. About 20 feet of silked copper wire were made into a short ring helix, on one end of a pasteboard tube, through which a cylindrical magnet, an inch in diameter, could move freely; one end of the helix wire was fastened to a small amalgamated copper plate, and the other end bent round so as to touch this plate perpendicularly upon its flat surface, and also in such a manner that when the magnet was passed through the cylinder it should come against this wire, and separate the end from contact with the plate. The consequence was that whenever this action was quickly performed, the magneto-electric spark appeared at the place of disjunction.

My apparatus was placed horizontally, and a short loose plug of wood was put into the end of the cylinder, so that the disjunction at the plate should take place at the moment the end of the magnet was passing through the helix ring, that being the most favourable condition of the apparatus. The magnet was driven with a sharp quick motion through the cylinder, its impetus being overcome, as soon as the spark was obtained, by an obstacle placed at a proper distance on the outside of the moveable wire. From the brightness and appearance of the spark, I have no doubt that if both ends of a horse-shoe magnet were employed, and a jogging motion were communicated to the light frame carrying the helices, a spark equal, if not superior, to those which down to this time have been obtained with magnets of a certain power, would be produced.



Thus the magneto-electric spark has been brought one step nearer to the exciting magnet. The much more important matter still remains to be effected of rendering that electricity which is in the magnet itself, and gives it power, evident under the form of the spark.

The next point to which I wish to direct your attention is

the *magneto-electric shock*. This effect I have felt produced by Mr. William Jenkins in a manner that was new to me; and as he does not intend to work out the result any further, but has given me leave, through Mr. Newman, to make it known to you, I think the sooner it is published the better. Mr. Jenkins's apparatus consists of a helix cylinder formed of copper wire in the usual manner. An iron rod, about 2 feet long and half an inch in diameter, can be passed at pleasure into the centre of this cylinder. The helix consists of three lengths of wire, (which, however, might as well be replaced by one thick wire,) the similar ends of which are soldered to two thicker terminal wires, and on these are soldered also two short copper cylinders, to be held in the hand and give extensive contact. The electro-motor was a single pair of plates, exposing, perhaps, 3 square feet of surface on *both* sides of the zinc plate. On holding the two copper handles tightly in the hands, previously moistened with brine, and then alternately making and breaking the contact of the ends of the helix with the electro-motor, there was a considerable electric shock felt in the latter case, *i.e.* on breaking contact, provided the iron rod were in the helix; but none either on making or breaking contact when the latter was away.

This effect appears very singular at first, in consequence of its seeming to be the shock of the electricity of a single pair of plates. But in reality it is not so. The shock is not due to the electricity set in motion by the plates, but to a current in the reverse direction, induced by the soft iron electro-magnet at the moment when, from the cessation of the original current, it loses its power. It is, however, very interesting thus to observe an original current of electricity, having a very low intensity, producing ultimately a counter current having an intensity probably a hundredfold greater than its own, and the experiment constitutes one of the very few modes we have at command of converting quantity into intensity as respects electricity in currents.

It has been generally supposed that the electric spark producible by a single pair of plates can only be obtained upon breaking contact; but this, as I have shown in the Eighth Series of my Experimental Researches, is an error, and a very important one as regards the theory of voltaic electricity; it is, however, true that the spark upon breaking contact can be very greatly exalted by circumstances having no such effect upon that produced at the moment of making contact.

Every experimenter on electro-magnetism is aware, that when the current from a single pair of plates is passed through a helix surrounding a piece of soft iron (to produce an electro-

magnet,) the spark, upon breaking contact, is much brighter than if the soft iron were away; and because this effect occurs at the same moment with the shock in Mr. Jenkins's experiment, it might at first be supposed that the electricity producing both the spark and the shock was the same, and that the effects of both were increased, because of the increase in power of this their common cause. But the fact is not so, for the electricity producing the spark is passing in one direction, being that which the zinc plate and acid determine, whilst the electricity producing the shock is circulating in the contrary way.

From the appearance of the spark, which is always in this form of the experiment due to the electricity which is passing at the moment when contact is broken, it might seem that a greater current of electricity is circulating during the time that the contact is preserved, whilst the iron is present in the helix, than when it is away. But this is not the case; for when the quantity is measured by a very delicate galvanometer, it is found to remain unchanged after the removal or replacement of the iron, and to depend entirely upon the action at the zinc plate. Still the appearance of the spark is an evident and decisive proof, that the electricity which is passing at the moment of disjunction is of greater intensity when the iron is in the helix than when it is away, and this increased effect is evidently dependent, not upon any change in the state of things at the source of the electricity, but in a change of the powers of the conducting wire caused by the presence of the soft iron. I do not suppose that this change is *directly* connected with the magnetizing power of the current over the iron, but is due rather to the power of the iron after it becomes a magnet, to react upon the wire; and I have no doubt, though I have not had time to make the experiment, that a magnet of very hard steel, of equal force with the soft iron magnet, if put into the helix in the same direction, would exert an equal influence over the wire.

I will now notice another circumstance, which has a similar influence in increasing the intensity of the spark which occurs when the junction of the circuit is broken. If a pair of zinc and copper plates immersed in acid are connected by a short wire, and all precautions are taken to avoid sources of inaccuracy, then, as I have already shown, the spark, upon breaking contact, is not greater than that upon making contact. But if the connecting wire be much lengthened, then the spark upon breaking contact is much increased. Thus, a connecting copper wire of $\frac{1}{18}$th of an inch in diameter when 12 inches long, produced but a small spark with the same pair of plates

which the moment before or after would give a large spark with a wire of the same diameter and 114 feet long. Again, 12 inches in length of wire $\frac{1}{9}$th of an inch in diameter gave a much smaller spark than 36 feet of the same wire.

In both these cases, though the long wires gave the larger spark, yet it was the short wires which conducted the greatest quantity of electricity in a given time; and that was very evident in the one of small diameter, for the short length became quite hot from the quantity of electricity passing through it, whereas the larger wire remained cold. Still there can be no doubt that the sparks from the long wires were of greater intensity than those from the short wires, for they passed over a greater interval of air; and so the paradoxical result comes forth, that currents of electricity having the same common source, and passing the same quantity of electricity in the same time, can produce in this way sparks of very different intensity.

This effect, with regard to lengthened wires, might be explained by assuming a species of momentum as being acquired by the electricity during its passage through the lengthened conductor, and it was this idea of momentum which guided Signori Nobili and Antinori in their process for obtaining the magneto-electric spark by means of a common magnet. Whether a current of electricity be considered as depending upon the motion of a fluid of electricity or the passing of mere vibrations, still the essential idea of momentum might with propriety be retained. But it is evident that the similar effect produced by the soft iron of increasing the intensity of the spark cannot be explained in this way, *i. e.* by momentum; and as it does not seem likely that the effects, which in these cases are identical, should have two causes, I believe that both are produced in the same way, although the means employed are apparently so different.

When the electric current passes through a wire, that wire becomes magnetic; and although the direction of the magnetism is peculiar, and very different to that of the soft iron placed in the helix of the first experiments, yet the direction of the magnetic curves, both of the wire so magnetized and of the soft iron magnet, in relation to the course which the current is pursuing (*i. e.* in the conducting wire), is the same. If, therefore, we refer the increased spark to a peculiar effect of induction exerted by the magnetism over the passing electric current, all becomes consistent. Let us, for instance, for the sake of reference, represent the magnetism by the magnetic curves: then, in the first place, the longer the wire the greater

the number of magnetic curves which can exert their inductive influence; and the effect in a wire of a hundred feet in length will be nearly a hundred times greater than in a wire of the same diameter only a foot in length. The reason why a core of soft iron produces the same effect as elongation of the wire, will be that it also brings magnetic curves into inductive action exactly in the same direction as those around the wire; and the rest of the circumstances, as far as I can perceive, will accord with the cause assumed.

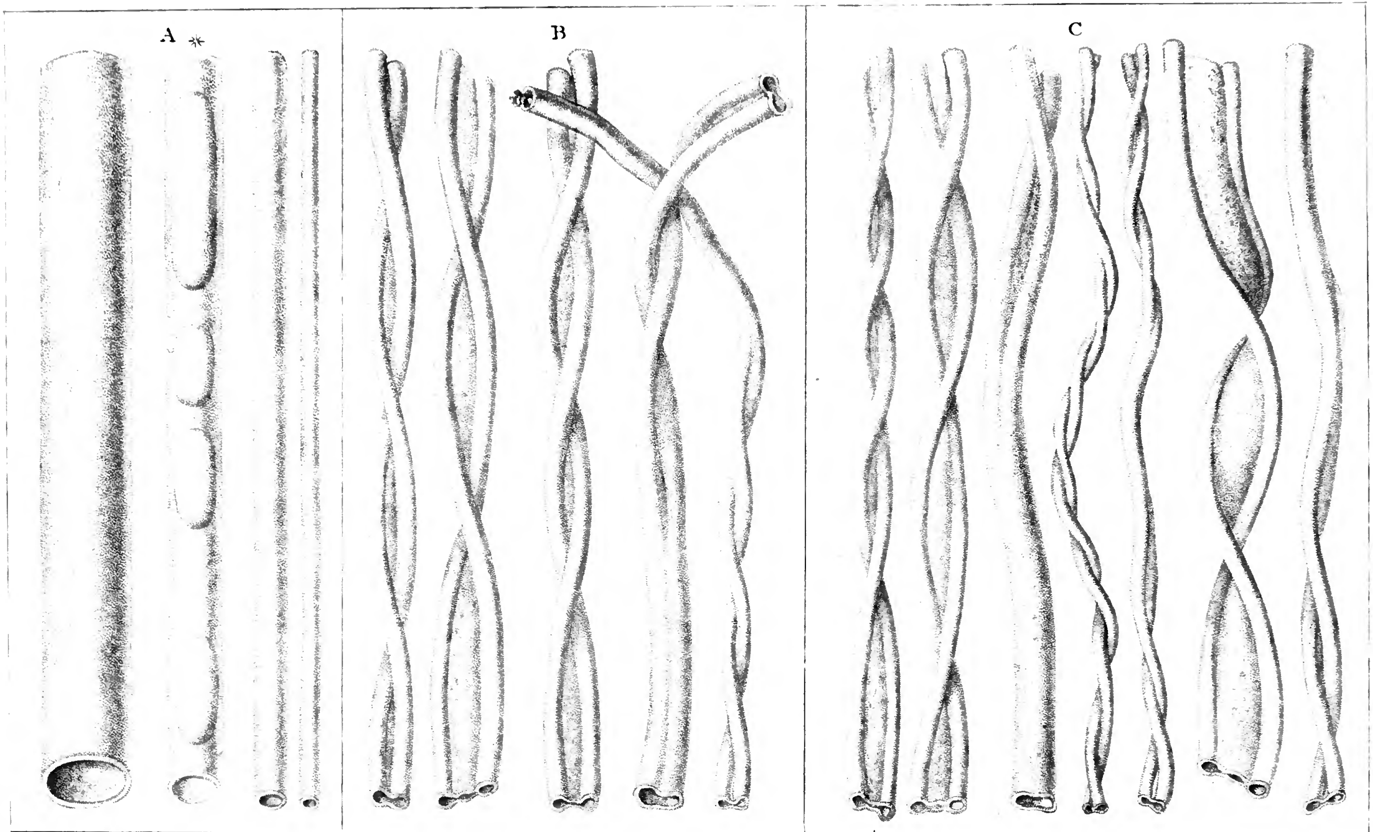
That the magnetic curves of the wire carrying the current shall actually affect the character of the current which gives them origin, need not excite any difficulty, for this branch of science shows many such cases. Ampère's experiment of revolving a magnet on its own axis, and the case which I have shown of drawing away electricity from the poles and equator of a magnet when it is revolved, are both instances of the same kind.

In conclusion, I wish to say that I think I see here some of those indications of an *electro-tonic* or peculiar state of which I have expressed expectations in the second series of my Experimental Researches, par. 242.*; for though I here speak of magnetism and magnetic curves for the sake of reference, yet allowing Ampère's theory of the magnet, all the effects may be viewed as effects of induction produced by electrical currents. Hence many extensions of the experiments. I have no doubt, for instance, that if a long wire were arranged so as to discharge a single pair of plates, and the spark occurring at the breaking of contact were noted, and then another wire carrying a current in the same direction from another electrometer, were placed parallel and close to but without touching the first, the spark obtained on breaking the contact at the first wire would be greater than before. This experiment can easily be made with a double helix; but at my present distance from town I have no means of trying the experiment, or of examining more closely these indications.

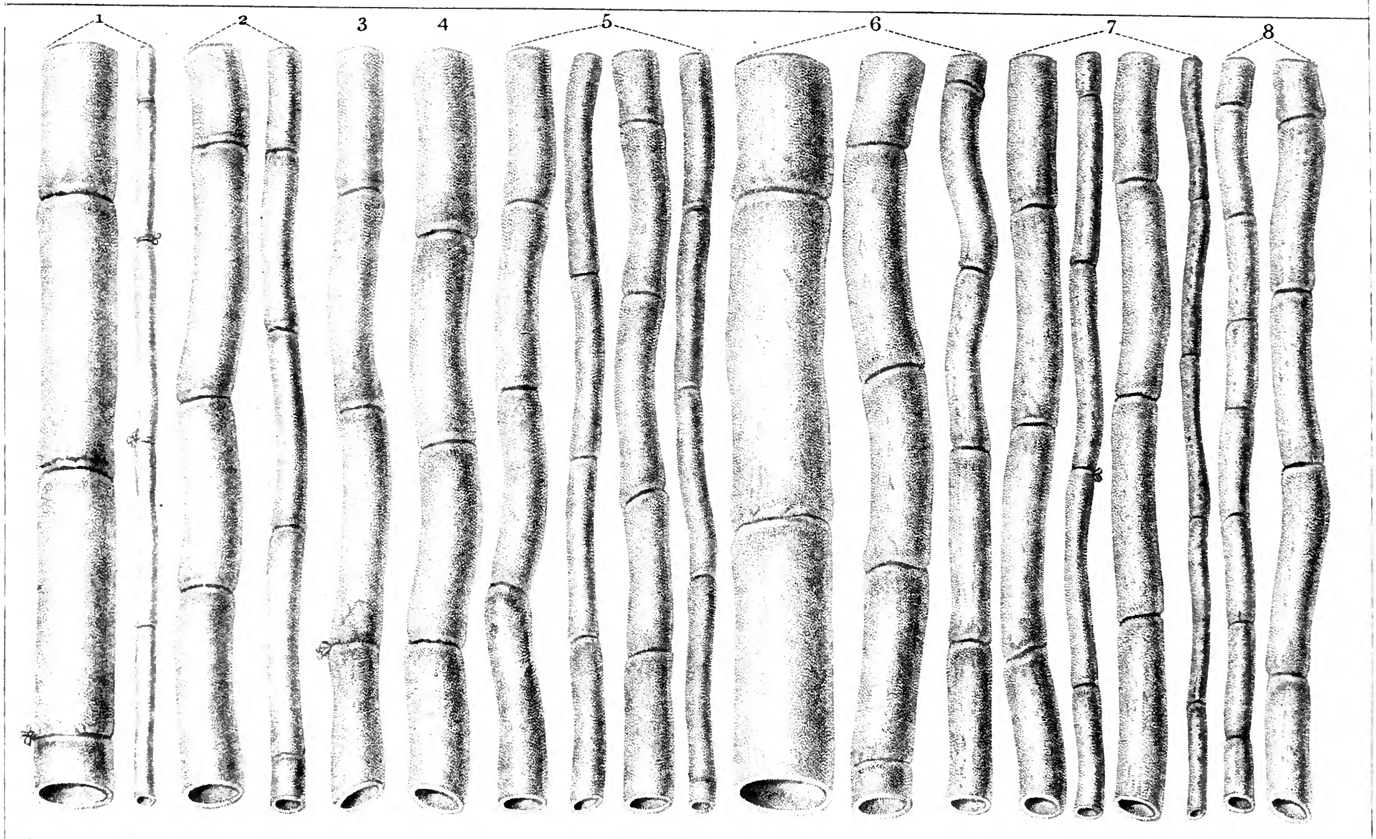
I am, my dear Sir, very truly yours,

Brighton, Oct. 17, 1834. M. Faraday.

* Philosophical Transactions 1832, p. 189.



Kew-Green, Feb.y 11th 1822



Francis Bauer del. Kew Green, April 24th 1832. F. Bauer sc.

XLVIII. *On the Mummy Cloth of Egypt; with Observations on some Manufactures of the Ancients.* By JAMES THOMSON, *Esq., F.R.S.**

[With a Plate.] The originals for this are in Dept. of Botany

B

§ I.

THE inquiries which form the subject of the following paper were undertaken many years ago: circumstances which it is unnecessary here to explain have delayed their publication; but the results were communicated to numerous individuals. The revival lately of similar inquiries by others apparently unacquainted with what is already known, induces me to believe that this communication may not be wholly without interest.

My attention was attracted to the subject of Egyptian manufactures by the late Mr. Belzoni in the year 1822, during the exhibition of a model of the ancient tomb discovered by that enterprising traveller in Egypt. He had the goodness to present to me various specimens of cloth, chiefly from the mummies in his possession, one of which he had entirely denuded.

On my remarking that these fabrics scarcely deserved the appellation of "fine linen," which from all antiquity had been bestowed on the linen of Egypt, and that the observations of Dr. Hadley, in the Philosophical Transactions for the year 1764, had thrown some doubt on the supposed fineness of this linen, he informed me that during his researches in Egypt, in those tombs and mummy-pits which he had explored, he had met with cloth of every degree of fineness from the coarsest sacking to the finest and most transparent muslin, a fact which I subsequently found in a great degree confirmed by the acquisition of some interesting specimens of mummy cloth sent to this country by the then Consul-general of Egypt, the late Mr. Salt. The subject appearing to me sufficiently interesting to deserve investigation, and having collected a variety of specimens of cloth, my first care was to ascertain of what material they were made. This question had already engaged the attention of various inquirers and given birth to learned dissertations.

Rouelle, in the Memoirs of the French Academy of Sciences for the year 1750; Larcher, the translator of Herodotus, in the notes to that celebrated work; and the learned John Reinhold Forster, who wrote a tract *De Bysso Antiquorum*, had all endeavoured to prove from their own examination that

* Communicated by the Author.

the mummy cloth of Egypt was cotton; and this opinion, on their authority, was adopted by the learned of Europe. It is singular that neither in the memoir of Rouelle, nor in the notes of Larcher, nor in the dissertation of Dr. Forster, in which this opinion is expressed, are any grounds assigned for, or any proofs given of, this opinion. The amount of their assertion is, that having examined the bandages of various mummies, which are designated by them, and some of which I have myself since carefully examined, they found all those which were free from resinous matter to be cotton. I am forced to confess that with all the attention I could bestow upon them, and with the assistance of various intelligent manufacturers, I was unable to arrive at such a conclusion. Some were of opinion that the cloth was cotton; others that it was linen; and some, again, that there were in the collection specimens of both,—a proof that our means of judging were unworthy of confidence.

The great difference in the specific gravities as well as in the conducting power of linen and cotton is sufficient to enable us, by careful experiments, to discriminate accurately between them; and there are few individuals who have been accustomed to the use of both cotton and linen who cannot readily distinguish, by that delicate sense of touch diffused over the whole body, between the two fabrics: but such tests require much larger portions of the material than I had at my disposal, many of the specimens submitted to my examination not being larger than a shilling. I found the difference of smell in the burnt fibres, and the degree of polish which each kind of cloth took on being rubbed with a glass stopper, as well as other empirical modes suggested to me, liable to great uncertainty, and I sought in vain for any chemical test. It occurred to me that the supposed unfitness of cotton lint, compared with linen, for dressing wounds, had been accounted for by the different form of their fibres, the one being sharp and angular, and the other round and smooth; and, in fact, I found in the 12th volume of the Philosophical Transactions, for the year 1678, this structure ascribed to them by that early microscopic observer Mr. Leuwenhoek. It seemed to me, therefore, that the most simple mode of distinguishing between cotton and linen would be to subject the fibres to examination under a powerful microscope. Not being possessed of such an instrument, nor accustomed to its management, my friend Mr. Children undertook, through Sir Everard Home, to solicit the assistance of Mr. Bauer, whose labours are well known to the scientific world, and whose microscopic drawings have for a series of years enriched the Transactions of the Royal

Society. I transmitted to him various fibres of cotton and linen, both manufactured and in their raw state, as well as fibres of unravelled mummy cloth, and in a few days I received from him a letter, in which he pronounced every specimen of mummy cloth subjected to his examination to be linen.

This letter was accompanied by a beautiful drawing, exhibiting the fibres of both raw and unravelled cotton as flattened cylinders, twisted like a corkscrew, whilst the fibres of linen and various mummy cloths were straight and cylindrical.

Repeated observations having established beyond all doubt the power of the microscope accurately to distinguish between the fibres of cotton and linen, I obtained, through the kindness of various individuals connected with the British Museum, the Royal College of Surgeons, the Hunterian Museum of Glasgow, as well as other public institutions, both at home and abroad, a great variety of cloths of human mummies, and of animals and birds, which being subjected to the microscope of Mr. Bauer, proved without exception to be linen; nor has he, amongst the numerous specimens we have both collected during many years, been able to detect a single fibre of cotton; a fact since recently confirmed by others, and proving incontestibly that the mummy cloth of Egypt was linen.

§ II.

The filaments of cotton when viewed through a powerful instrument, such as the improved achromatic microscope of Ploessl of Vienna, which for magnifying power and clearness of vision Mr. Bauer has found superior to every other he has had an opportunity of using, appear to be transparent glassy tubes, flattened, and twisted round their own axis. A section of the filament resembles in some degree a figure of 8, the tube originally cylindrical having collapsed most in the middle, forming semi-tubes on each side, which give to the fibre, when viewed in certain lights, the appearance of a flat ribbon with a hem or border at each edge. The uniform transparency of the filament is impaired by small irregular figures, in all probability wrinkles or creases arising from the desiccation of the tube. The twisted and corkscrew form of the filament of cotton distinguishes it from all other vegetable fibres, and is characteristic of the fully ripe and mature pod, Mr. Bauer having ascertained that the fibres of the unripe seed are simple untwisted cylindrical tubes, which never twist afterwards if separated from the plant; but when the seeds ripen, even before the capsule bursts, the cylindrical tubes collapse in the middle and assume the form already de-

scribed, and which is accurately delineated in the accompanying drawing.

This form and character the fibres retain ever after, and in that respect undergo no change through the operation of spinning, weaving, bleaching, printing, and dyeing, nor in all the subsequent domestic operations of washing, &c., till the stuff is worn to rags; and then even the violent process of reducing those rags to pulp for the purpose of making paper, effects no change in the structure of these fibres. "With Ploessl's microscope," says Mr. Bauer, "I can ascertain whether cotton rags have been mixed with linen in any manufactured paper whatever."

The elementary fibres of flax (*Linum usitatissimum*) are also transparent tubes, cylindrical, and articulated or jointed like a cane. This latter structure is only observable by the aid of an excellent instrument. They are accurately delineated in the annexed engraving.

Explanation of the Plate.

First row of figures: A. Fibres of the unripe seed of cotton. In that state the fibres are perfect cylindrical tubes. At * is a fibre represented as seen under water, showing that the water had gradually entered and inclosed several air-bubbles, proving the tube to be quite hollow and without joints.

B. The first two fibres are from ripe cotton and are already twisted, though the pod or capsule is not yet burst, and is still on the growing plant. The other three fibres are of raw cotton prepared for manufacture.

C. Various fibres of unravelled threads of manufactured cotton. The fibres of cotton in the annexed drawing are represented $\frac{1}{100}$ of an inch in length, and are magnified 400 times in diameter. In thickness these fibres vary from $\frac{1}{800}$ to $\frac{1}{3600}$ part of an inch. The twists or turns in a fibre of cotton are from 300 to 800 in an inch.

Second row of figures:

Fig. 1. Fibres of raw flax before spinning.

Fig. 2. Fibres of unravelled threads of manufactured flax.

Figg. 3, 4, 5. Fibres of the unravelled threads of various mummy cloths.

Fig. 6. Fibres of unravelled threads of the cloth of Dr. Granville's mummy, supposed to be cotton. The specimens are all flax, and the fibres remarkably strong and large.

Fig. 7. Fibres of unravelled threads of several Ibis mummies.

Fig. 8. Fibres of unravelled threads of the mummy of an ox's head.

All the annexed figures of fibres of flax represent each $\frac{1}{100}$ of an inch in length, and are magnified 400 times in diameter. They vary in thickness from $\frac{1}{800}$ to $\frac{1}{3800}$ part of an inch.

§ III.

Of the productions of the loom amongst the nations of antiquity, with the exception of those which form the subject of this paper, we know only what is to be gathered from the few scattered notices in ancient writers. Even the great work of Pliny, the encyclopædia of that day, and with all its defects an invaluable collection of facts, affords but scanty information. Of the manufactures of the Egyptians and of their domestic arts our knowledge is more ample, but we are more indebted to their monuments than to their historians; and the paintings which adorn their tombs, and which are fresh at the present day as from the hand of the artist, have revealed to us more than all the writers of antiquity.

Of the products of the Egyptian loom, however, we know scarcely more than the mummy-pits have disclosed to us; and it would be as unreasonable to look through modern sepulchres for specimens and proofs of the state of manufacturing art amongst ourselves, as to deduce an opinion of the skill of the Egyptians from those fragments of cloth which envelop their dead, and have come down, almost unchanged, to our own time. The curious or costly fabrics which adorned the living, and were the pride of the industry and skill of Thebes, have perished ages ago. There are, however, amongst these remains some which are not unworthy of notice, which carry us back into the workshops of former times, and exhibit to us the actual labours of the weavers and dyers of Egypt more than two thousand years ago.

The great mass of the mummy cloth employed in bandages and coverings, whether of birds, animals, or of the human species, is of coarse texture, especially that more immediately in contact with the body, and which is generally impregnated with resinous or bituminous matter. The upper bandages, nearer the surface, are finer. Sometimes the whole is enveloped in a covering coarse and thick, and very like the sacking of the present day; sometimes in cloth coarse and open, like that used in our cheese-presses, for which it might easily be mistaken. In the College of Surgeons are various specimens of these cloths, some of which are very curious.

The beauty of the texture and peculiarity in the structure of a mummy cloth given to me by Mr. Belzoni was very striking. It was free from gum, or resin, or impregnation of any kind, and had evidently been originally white. It was

close and firm, yet very elastic. The yarn of both warp and woof was remarkably even and well spun. The thread of the warp was *double*, consisting of two finer threads twisted together. The woof was single. The warp contained 90 threads in an inch; the woof, or weft, only 44. The fineness of these materials, estimated after the manner of cotton yarn, was about 30 hanks in the pound.

The subsequent examination of a great variety of mummy cloths showed that the disparity between the warp and woof belonged to the system of manufacture, and that the warp generally had twice or thrice, and not seldom four times, the number of threads in an inch that the woof had: thus, a cloth containing 80 threads of warp in the inch, of a fineness about 24 hanks in the pound, had 40 threads in the woof; another with 120 threads of warp of 30 hanks had 40; and a third specimen only 30 threads in the woof. These have each respectively double, treble, and quadruple the number of threads in the warp that they have in the woof. This structure, so different from modern cloth, which has the proportions nearly equal, originated, probably, in the difficulty and tediousness of getting in the woof when the shuttle was thrown by hand, which is the practice in India at the present day, and which there are weavers still living, old enough to remember the universal practice in this country.

I have alluded to some specimens of mummy cloth sent to this country by the late Mr. Salt. I am unacquainted with their history or origin further than that they were brought from Thebes, and were contained in the outer packing-case of a mummy now in the British Museum. They were evidently the spoils of some other mummy, but when and where opened I have in vain endeavoured to learn. There were various fragments of different degrees of fineness; some fringed at the ends, and some striped at the edges. They merit a more particular description.

My first impression on seeing these cloths was that the finest kinds were *muslin*, and of Indian manufacture, since we learn from the "Periplus of the Erythrean Sea," ascribed to Arrian, but more probably the work of some Greek merchant himself engaged in the trade, that muslins from the Ganges were an article of export from India to the Arabian Gulf; but this suspicion of their being cotton was soon removed by the microscope of Mr. Bauer, which showed that they were all without exception linen. Some were thin and transparent, and of very delicate texture. The finest appeared to be made of yarns of near 100 hanks in the pound, with 140 threads in the inch in the warp, and about 64 in the woof. A specimen

of muslin in the Museum of the East India House, the finest production of the Dacca loom, has only 100 threads in an inch in the warp, and 84 in the woof, but the surprising fineness of the yarns, which, though spun by hand, is not less than 250 hanks in the pound, gives to this fabric its unrivalled tenuity and lightness.

Some of the cloths were fringed at the ends, and one, a sort of scarf about four feet long and twenty inches wide, was fringed at both ends. Three or four threads twisted together with the fingers to form a strong one, and two of these again twisted together and knotted at the middle and at the end to prevent unravelling, formed the fringe, precisely like the silk shawls of the present day.

The selvedges of the Egyptian cloths generally are formed with the greatest care, and are well calculated by their strength to protect the cloth from accident. Fillets of strong cloth or tape also secure the ends of the pieces from injury, showing a knowledge of all the little resources of modern manufacture. Several of the specimens, both of fine and coarse cloth, were bordered with blue stripes of various patterns, and in some alternating with narrow lines of another colour. The width of the patterns varied from half an inch to an inch and a quarter. In the latter were seven blue stripes, the broadest about half an inch wide nearest the selvedge, followed by five very narrow ones, and terminated by one an eighth of an inch broad. Had this pattern, instead of being confined to the edge of the cloth, been repeated across its whole breadth, it would have formed a modern gingham, which we can scarcely doubt was one of the articles of Egyptian industry. A small pattern about half an inch broad formed the edging of one of the finest of these cloths, and was composed of a stripe of blue followed by three narrow lines of the same colour, alternating with three lines of a fawn colour, forming a simple and elegant border. These stripes were produced in the loom by coloured threads previously dyed in the yarn. The nature of the fawn colour I was unable to determine. It was too much degraded by age, and the quantity too small to enable me to arrive at any satisfactory conclusion. Though I had no doubt the colouring matter of the blue stripes was indigo, I subjected the cloth to the following examination. Boiled in water for some time, the colour did not yield in the least; nor was it at all affected by soap, nor by strong alkalies. Sulphuric acid, diluted only so far as not to destroy the cloth, had no action on the colour. Chloride of lime gradually reduced, and at last destroyed it. Strong nitric acid dropped upon the blue turned it orange, and in the same instant destroyed it.

These tests prove the colouring matter of these stripes to be indigo.

This dye was unknown to Herodotus, for he makes no mention of it. It was known to Pliny, who, though ignorant of its true nature and the history of its production, has correctly described the most characteristic of its properties, the emission of a beautiful purple vapour when exposed to heat. Had his commentators been acquainted with the sublimation of indigo, it would have saved many learned doubts. We learn from the "Periplus," that it was an article of export from Barbariké on the Indus to Egypt, where its employment by the manufacturers of that country, probably from a remote period, is clearly established by the specimens here described.

Amongst the various cloths for which I am indebted to the curators of the Hunterian Museum at Glasgow, is one of a pale brick or red colour. My attention was lately recalled to this specimen by observing a similar colour in the outer coverings of two fine mummies presented to the University of London by Mr. Morrison, one of which has been recently unrolled. Having obtained specimens of both, I subjected them, with that from Glasgow, to the following experiments. Treated with cold water the colour was not affected. Boiling distilled water in a few minutes nearly removed the whole. Diluted sulphuric or muriatic acid had no action on it; but a feeble alkali, whether carbonated or caustic, destroyed the colour immediately. Examined with a lens, the specimens from Glasgow exhibited small distinct grains or concretions, of a red colour, disseminated through the fibres of the cloth. Notwithstanding the fugitive nature of the colouring matter of Safflower, the *Carthamus tinctorius* of botanists, I am strongly disposed to consider the three specimens here examined as having been dyed with that plant. The small granular particles of a red colour observed in the Glasgow specimen are sometimes found in cloth dyed with *Carthamus.* There is also in the covering of the mummy of the London University which is unstripped, a rosy hue peculiar to this dye. The resistance of the colour to acids and its instant yielding to the weakest alkalies is characteristic of Safflower. Lastly, *Carthamus* has long been an article of cultivation in Egypt, and the first processes employed by the European dyers were derived, with the dye itself, from that country, where in all probability it has been cultivated and used for ages, and is to this day an article of considerable export.

In the Glasgow mummy there was, moreover, a narrow slip of cloth about four inches broad, extending from the crown of the head to the feet, of a yellowish colour, of which

portions were still fresh. On examination no mordant appeared to have been used to fix this dye, and washing in cold water greatly impaired it. Comparative experiments made on this colour, and on that afforded by *Carthamus* to simple water before the pink dye is extracted, left little doubt of their being identical. They were slightly and similarly affected by solutions of alumina and of iron, and appeared to have very feeble affinities for either vegetable fibre or any of the earthy or metallic bases.

Though the age of the mummies from which these specimens were derived has not been ascertained, yet we may fairly presume that it goes back to a period so far remote as to make the preservation so long, of delicate and fugacious colouring matter like *Carthamus*, or even the more permanent one of indigo, very surprising, and proves that substances which readily yield to the combined and destructive agency of heat or light and *moisture*, are almost unalterable when secured from the action of the latter. Portions of the blue cloth which had resisted in the dark and dry sepulchres of Thebes for ages, lost, by a few days' exposure on the grass, nearly all their colour.

Mummy cloth not stained or discoloured by resin or bitumen is generally of a pale-brown or fawn colour, which has been supposed to arise from some astringent preparation employed by the Egyptians for its preservation. All this cloth imparts to water a brown colour, in which I have sought in vain for any trace of tannin. In none of the specimens I have examined did either gelatine or albumen, or solutions of iron, afford any precipitate; but the subacetate of lead produced a cloud, indicating the presence of extractive matter. I am inclined to think that if astringent matter has been found, it is in those bandages which have received a preparation of gum or resin, and which are distinguished from the others by their stiffness. These I have not examined. All these cloths, whether fine or coarse, are more or less rotten. Of the numerous specimens which have fallen under my notice, the outer covering of the fine mummy in the London University has suffered least: it is comparatively sound. Whether this be an argument against its high antiquity, I know not; but the cloth is evidently ancient Egyptian: nor is it, I believe, pretended that in those factitious mummies manufactured by the Arabs, of which several were found by Blumenbach in the British Museum, the bandages and envelopes are not genuine. Of the ancient cloth there is such an accumulation in the mummy pits and sepulchres of Egypt, as to have become an object of speculation in Europe, for the

purpose of making paper. The inquiries, therefore, which form the subject of this communication are not affected by any question of the integrity of those mummies from whence the specimens were derived, of which, however, no doubt is entertained.

The period during which the custom of embalming prevailed in Egypt, embraces a long succession of ages. From the first of the Pharaohs to the last of the Ptolemies, with whom this ancient rite is supposed to have become almost extinct, chronologists reckon more than twenty centuries during which the art was practised which has handed down to us these scanty remains of Egyptian industry, the only vestiges of the labours of the ancient loom now in existence. They prove the arts of spinning and weaving flax to have attained a high degree of perfection, many of the specimens of mummy cloth here described being of a quality to excite admiration even at the present day, and the finest of these fabrics approaching in excellence our delicate muslins. The coloured borders establish the fact of indigo having been known and used as a dye in Egypt, from a remote æra.

During this long period, industry and the arts of life connected with civilization must have made considerable progress, which we shall, however, remain unable satisfactorily to trace till more accurate knowledge of the ancient language and characters of the Egyptians shall have interpreted the dates, and fixed the chronology of their monuments and paintings. In the tomb of Beni Hassan is a representation of a loom (figured in Count Minutoli's Travels) of such primæval simplicity as to resemble the first rude efforts of savage art to form a web, such as Don Ulloa in his voyages has described as used by the native Indians of South America. Between this loom, and that in which the corslet of Amasis was woven, mentioned by Herodotus, and more particularly described by Pliny as a wonderful specimen of manufacturing art, the distance is immense.

It is not improbable that future researches directed to this object may discover, in the ancient sepulchres and mummy pits, fragments of cloth, now trodden under foot and unheeded by the traveller, which would throw much light on the interesting subject of ancient manufactures.

The question debated amongst the learned of the nature of the Byssus of the ancients, I may in conclusion be permitted to observe, appears to me to be finally settled by the present communication. Herodotus states that the Egyptians wrapped their dead in cloth of the *Byssus*. It has been shown that without exception every specimen of mummy cloth yet ex-

amined has proved to be linen. We owe, therefore, the satisfactory establishment of the fact, that the *Byssus* of the ancients was FLAX, to the microscope of Mr. Bauer.

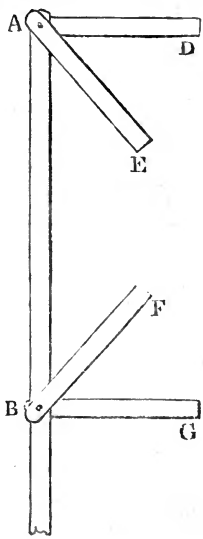
XLIX. *A Method of determining the Number of Signals which can be made by the Modern Telegraphs.* By CHARLES BLACKBURN, *Esq., B.A.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN the Number of the Philosophical Magazine for October, you did me the favour to insert a method of finding the number of signals which can be made by semaphores having one arm on a centre; I now send an investigation for finding the signals when the semaphores have any given number of arms on the same centre.

Problem.— To find the number of signals which can be made by a semaphore having any number of centres; any given number of arms on each centre; and each arm taking any given number of positions.



Let the figure represent a semaphore having any number of centres, A, B, &c., and any number of arms, AD, AE, &c., on the same centre; let also the number of centres be denoted by c, the number of arms by a, and the number of the positions of each arm $= p$.

First, to find the number of signals which can be made by using the arms on one centre only.

The number of positions in which each arm can be placed may be considered as distinct quantities capable of a certain number of combinations. The number of positions of each arm being p, the number of signals which can be made with one arm only must $= p$.

And since the number of combinations of p things, taken two and two together, is $\frac{p \cdot \overline{p-1}}{1 \cdot 2}$, the number of signals, using two arms only, will be $\frac{p \cdot \overline{p-1}}{1 \cdot 2}$. In like manner the number,

when three arms are made use of, will be $\frac{p \cdot \overline{p-1} \cdot \overline{p-2}}{1 \cdot 2 \cdot 3}$; and when all the arms on the centre are used, the signals will be $\frac{p \cdot \overline{p-1} \ldots \{\overline{p-a-1}\}}{1 \cdot 2 \ldots\ldots\ldots \overline{a-1} \cdot a}$. Hence the total number of signals which can be made by the arms on a single centre will be represented by the sum of the series

$$p + \frac{p \cdot \overline{p-1}}{1 \cdot 2} \; \frac{p \cdot \overline{p-1} \cdot \overline{p-2}}{1 \cdot 2 \cdot 3} + , \&c. \ldots\ldots \frac{p \cdot \overline{p-1} \ldots \{\overline{p-a-1}\}}{1 \cdot 2 \ldots\ldots \overline{a-1} \cdot a}$$

to a terms.

Let the sum of this series be denoted by A; then since the number of centres $= c$, the whole number of signals which can be made, using the arms on a single centre at a time, $= c\,\text{A}$.

Again, since the signals which can be made on any one centre can be combined with each of the signals on any other centre, it follows that the number which can be made, using any two centres at a time, $= \text{A}^2$.

Also, since the signals which can be made upon any two centres may be combined with each of the signals upon any other centre, it follows that the number of signals, using any three centres at once, will be A^3.

In like manner it may be shown that the signals, using c centres at once, will be A^c. Hence

The number of signals using one centre at a time $= \text{A}$
———————— two centres.......... $= \text{A}^2$
&c. &c. &c.
———————— c centres .... $= \text{A}^c$

Again, since the number of combinations of c things, taken two and two together, $= \frac{c \cdot \overline{c-1}}{1 \cdot 2}$, it follows that the number of different pairs of centres which can be taken together will be $\frac{c \cdot \overline{c-1}}{1 \cdot 2}$; and since the number of signals with each pair of centres $= \text{A}^2$, the total number of signals which can be made, using a pair of centres at once, will be $\frac{c \cdot \overline{c-1}}{1 \cdot 2} \cdot \text{A}^2$.

Also, since the number of combinations of c things, taken three and three together, is $\frac{c \cdot \overline{c-1} \cdot \overline{c-2}}{1 \cdot 2 \cdot 3}$, the number of dif-

ferent sets of three centres will be $\frac{c.\overline{c-1}.\overline{c-2}}{1.2.3}$; and since the number of signals which can be made upon any three centres is A^3, it follows that the total number of signals, when three centres are used at once, will be $\frac{c.\overline{c-1}.\overline{c-2}}{1.2.3}A^3$, &c.

In like manner, the numbers, when c centres are used at once, will be $\frac{c.\overline{c-1} \ldots c.\overline{c-1} \ldots\ldots \{c-\overline{c-1}\}}{1.2 \ldots\ldots c-1).c} A^c$.

It appears, then, that the whole number of signals which can be made by the instrument, will be represented by the sum of the series

$$cA+\frac{c.\overline{c-1}}{1.2}A^2+, \&c. \ldots\ldots \frac{c.\overline{c-1} \ldots \{c-\overline{c-1}\}}{1.2 \ldots\ldots\ldots c} A^c$$

to c terms, in which expression $A = p+\frac{p.\overline{p-1}}{1.2}+$, &c. to a terms. But by the binomial theorem, the sum of the series,

$$cA+\frac{c.\overline{c-1}}{1.2}A^2+\&c. \quad c. \quad \ldots\ldots \frac{c.\overline{c-1} \ldots \{c-\overline{c-1}}{1.2 \ldots\ldots c} A^c$$
$$= (A+1)^c-1;$$

hence the number of signals will be represented by the formula $(A+1)^c-1$.

Example.— Let it be required to find the number of signals which can be made by the Admiralty semaphore.

In this example $a = 1$, $c = 2$, $p = 6$; and since $a = 1$, $A = a$, and the expression $(A+1)^c-1$ becomes $(6+1)^2-1 = 48$, the number required.

Example 2.—To find the number of signals which could be made by the Admiralty semaphore supposing it to have two arms upon a centre.

Here $a = 2$, $\therefore A = p.\frac{p-1}{2} = \frac{6.5}{2}$, hence the signals will be $(15+1)^2-1 = 255$.

Corollary.—The number of signals which can be made, using any one centre, will be represented by the second term of the binomial $A+1$ raised to the cth power; the number using two centres, by the third term of the same power; and so on. Thus,

The signals using one centre at a time $=$ $c\,A$.

——— using two centres ...... $= \dfrac{c\,.\,\overline{c-1}}{1\,.\,2}\,.\,A^2$.

——— using three centres ...... $= \dfrac{c\,.\,\overline{c-1}\,.\,\overline{c-2}}{1\,.\,2\,.\,3}\,.\,A^3$.

&c. &c. &c.

——— using c centres $= \dfrac{c\,.\,\overline{c-1}\ \ldots\ \{c-\overline{c-1}\}}{1\,.\,2\ \ldots\ldots\ c}\,.\,A^c$.

Corollary 2.—In any semaphore which has as many arms upon each centre as each arm has positions, the formula for the number of signals will be $2^{ac}-1$.

For in this case, since

$$a = p,\ A = a + \frac{a\,.\,\overline{a-1}}{1\,.\,2} + \frac{a\,.\,\overline{a-1}\,.\,\overline{a-2}}{1\,.\,2\,.\,3} +, \text{\&c., to } a$$

terms $= 2^a - 1$. Hence by substituting for A in the formula $(A+1)^c - 1$ we have $(2^a)^c - 1 = 2^{ac} - 1$.

Example.—Let $a = p = 6$, $c = 2$; then $2^{ac}-1 = 2^{12}-1 = 4095$, which is the number of signals which could be produced by the Admiralty semaphore, if it had six arms upon each centre instead of one.

Example 2.—To find the number of signals which might be made with a pocket watch, supposing twelve instead of two fingers on the same centre.

Here $a = 12$, $c = 1$, therefore $2^{ac}-1 = 2^{12}-1 = 4095$, as in the preceding example.

All the signals requisite for the purposes of communication may be exhibited by machines of great simplicity. A telegraph like that in the annexed sketch is capable of producing sixteen thousand three hundred and eighty-three distinct signals; and since these admit of a systematic arrangement, by which the signal for every word, and the word to every signal, may immediately be found, it seems desirable that lines of telegraphs should be established between the metropolis and the more important towns. Communications of any nature might then be conveyed *verbatim* with the utmost facility, and with a rapidity approaching even to the velocity of light.

L. *Observations on the Growth and on the bilateral Symmetry of* Echinodermata. *By* L. Agassiz, *M.D. and Professor of Natural History at Neuchatel.**

THE most general character which has been usually assigned to the *Echinodermata*, is to have all the parts of their body similar to one another, and disposed like rays around a common centre: it is a character in which this class has been supposed to partake with the entire division of *radiated animals*. Nevertheless, on a close examination of this radiated structure, we find that these rays are always dissimilar, but in different degrees in different genera; and that they are not always connected with a centre of the same nature. If we trace the arrangement of parts in the *Spatangi*, for instance, we are soon led to see that the more or less elongated form of their body is caused by the position of the mouth and the anus, which are placed near the two extremities of the body; and that four of the ambulacral series, and also four of the interambulacral series, are pairs, and symmetrically placed on the two sides of a plane, which, if extended from the mouth to the anus, would divide the animal into two equal parts. The 5th ambulacral series, and also the 5th interambulacral series, are single, and not symmetrical. The ambulacral series which passes above the mouth (and which is odd, or not paired,) is consequently the anterior series; whilst the posterior part of the body is occupied by an interambulacral series: it is in the central line between its plates that the anus is placed. We have then in the *Spatangi* an anterior region, distinguishable by the unequal ambulacral series, and a posterior region, distinguishable by the unequal interambulacral series. On the two sides of the animal the series of plates are disposed in symmetrical pairs, in such a manner that there are two pairs of ambulacral series and two of interambulacral on the right, and two on the left. The first anterior pair, which adjoins the unequal ambulacral series, is a pair of interambulacral series, immediately behind which is placed the first pair of ambulacral series, then a second pair of interambulacral, and lastly, a second pair of ambulacral series. Behind these is the uneven posterior interambulacral series.

As to the *Clypeasters*, the *Galerites*, the *Nucleolites*, &c., in which the mouth is central and the anus marginal or submarginal, it is nevertheless easy to understand the position of the bilateral parts, because the position of the posterior interambulacral series being given by the position of the anus,

* Communicated by the Author.

there is no difficulty in understanding the symmetrical relations of the other even and odd series. We can always recognise differences in the form of the component plates, and of the ambulacra of the different pairs, which show evidently the appearance of bilateral parity. These, however, are less apparent in the *Clypeasters* than in the *Spatangi.* Hence these data become important for the study of the internal soft parts and for the appreciation of their functions.

It might seem that in passing to the *Echini* and *Asteriæ* (simple or ramified), whose mouth is perfectly central, and whose anus, when there is one, is likewise found in the middle, but upper part of the body, there would be no further traces of this bilateral symmetry, and yet even here it is easy to determine the relations of all the radiated parts and of the longitudinal anterio-posterior axis. All the radii of these animals resemble one another so much outwardly, that we might hardly expect to find in their arrangement traces of the bilateral symmetry which is so evident in the *Spatangi*, &c. But if we take into account the differences which exist in the structure of the plates of different series, we shall be convinced that here also the symmetry in pairs is maintained under the appearance of a disposition completely radiated; in fact, we find in the upper part of the disk of the *Echinodermata*, especially in the *Echini*, the *Cidarites*, &c., in the region where the ambulacral and interambulacral series converge, five perforated plates of peculiar form, which have been called oviducal plates, and are connected with the ovaries, and five interoviducal plates connected with the aquiferous system. The five largest of these plates (the oviducal ones) alternate with the extremities of the ambulacral series; four of these are, therefore, even, and one is odd. That which is odd has a porous particular structure; it is the madreporiform body of the *Asteriæ*, which equally exists in the *Echini*, but under another form: it is always in the posterior region of the body, and when it becomes imperceptible, the space where a lacuna is observed still points out the posterior region of the body, as the madreporiform body of the *Asteriæ* shows that the ray opposite to it is the odd anterior ray, whilst the four others are even, and placed on the right and left sides of the animal. The same is the case with the *Solasteriæ*, with this difference only, that in them the number of the pairs is more considerable, and that sometimes there is no uneven ray.

In order rightly to comprehend the mode of growth of the *Echinodermata*, it is necessary to keep in view the general disposition of the solid pieces that constitute their covering: they are plates of greater or less size, disposed in vertical

zones, diverging from the mouth to the periphery of the body, and converging from thence to the upper centre of the animal. The mouth obviously points out to us the anterior part of the body; but its ordinary position makes it look as if placed in the inferior part of the animal, but in no other respect changes the relations of the parts of the body to one another. We distinguish three principal types in the forms of these animals; some are tubular (the *Holothuriæ*), others spheroidal (the *Echinoides*), and others star-shaped (the *Asterides*); but they may be reduced to two types, as the tubular form may be viewed in this case as an elongated spheroid: still further, these two types may be reduced to one and the same plan of organization, since the enlargement or multiplication of the ovarial plates at the summits of such a spheroid, and of the plates around the mouth, accompanied by a contraction of the interambulacral plates, would produce a star, whilst, *vice versâ*, the enlargement of the extreme interambulacral plates, and the contraction of the central plates of a star, would produce a spheroid. This is not a mere supposition; the essential difference between the *Echini* and the *Asteriæ* consists in this different mode of increase of parts which are essentially the same. As to the general disposition of the plates in *Echinodermata*, there are generally twenty series of them, forming ten zones, of which one half are pierced with holes, whilst the other half are entire. The five zones or double series of perforated plates are called the ambulacral series; the others are the interambulacral series. In the Starfish (*Asteriæ*) the large plates of the sides of two adjacent radii answer to an interambulacral series of the *Echini*, whilst each radius has a complete ambulacral series extending from the mouth to the extremity of the radius, and thence back to the upper centre. The middle part of each ambulacral series, or the extreme point of each radius, is consequently the narrowest, and its two ends, or the basis of each radius, the widest. Each radius is, in fact, composed of two parts, resembling two isosceles triangles united by their summits and laid one over another; whereas in the *Echini* the centre of each series is the widest, and the extremities are the narrowest, like two triangles united by their bases at the equator of the sphere.

With respect to the original relations of all the plates which form these series, we should entertain a false idea if we represented them to ourselves as growing really in that vertical succession which they seem to possess. It is, indeed, at the summits of the series that the new plates are formed, but they succeed each other, like leaves in plants, spirally, from one in-

terambulacral series to another, so that those which lie in a vertical line one upon another, do not succeed each other in the order of their first growth.

The growth of the plates of *Echini* takes place chiefly at the upper summit of the shell, and the plates enlarge from the top towards the bottom until they arrive at the greatest circumference of the spheroid, where they become entirely consolidated: at the lower summit, *i. e.* around the mouth, the growth only takes place during the youth of the animal. Hence arise the differences of form which are observable between the young and the old *Echini*. In youth they are comparatively flat, and become more and more conical as they advance in age. In the Starfish the new plates are found in the angles of the radii nearest to the upper surface and lower surface of the body; and increasing more and more, they keep carrying to a greater distance the extremity of the radii. Thus, then, the number of the plates is continually increasing, and cannot be employed as a specific character. Hence we see how an *Echinus* or a Starfish receives its increase, still preserving its essential form and the relative disposition of its parts.

The same system prevails in the structure of the *Crinoidea*. In their internal organization it is well known that these animals had between their five rays a depression, which no doubt contained their soft organs; but the nature of this cavity is in part unknown, because the interposition of the rays usually obscures the view of their central region; yet after a close examination of many specimens of *Briarian Pentacrinite*, I have found that the branching rays are disposed around an inclosed cavity, having walls composed of plates distinct from those of the rays. The summit of this pouch presents an aperture surrounded with certain plates stronger than the rest. This aperture is the mouth, with its manducal plates. in many of the *Crinoidea*, particularly in the *Actinocrinites*, I have observed on the side of this cavity a second large aperture, placed between two rays of the animal. I consider this to be the anus; if so, this aperture occupies the same place as in the *P. europæus*, and presents a new resemblance to it. These data are sufficient to determine the bilateral disposition of the parts in the *Crinoidea*, as in the *Echini* and *Asteriæ*, and to point out which of the rays are pairs, and which is the odd ray. The differences which exist between them are so great, that in certain genera, *e. g.* in the *Pentremites* and *Actinocrinites*, they are obvious at the first glance.

As for the genera established in the whole class, I have

found that the characters drawn from the combination of the plates, and from the disposition of the ambulacra, form divisions more natural and better defined than the characters taken from the position of the mouth and of the anus.

I shall publish my detailed observations upon this subject in a monograph of the *Echinodermata*, accompanied with plates, for which I have already collected the greater part of the necessary materials.

LI. *An Account of a peculiar Optical Phænomenon seen after having looked at a moving Body, &c.* By R. ADDAMS, *Lecturer on Chemistry and Natural Philosophy**.

DURING a recent tour through the Highlands of Scotland, I visited the celebrated Falls of Foyers on the border of Loch Ness, and there noticed the following phænomenon.

Having steadfastly looked for a few seconds at a particular part of the cascade, admiring the confluence and decussation of the currents forming the liquid drapery of waters, and then suddenly directed my eyes to the left, to observe the vertical face of the sombre age-worn rocks immediately contiguous to the water-fall, I saw the rocky surface as if in motion upwards, and with an apparent velocity equal to that of the descending water, which the moment before had prepared my eyes to behold this singular deception.

The cascade is through a depth of about 70 feet, and my position, as I stood when I made the observation, was nearly on a level with the centre of the fall, being the lowest of the two situations where visitors obtain a view of this copious and never-failing infusion of peat† gushing over the giant step and whitening as it flows. My attention was engaged on that part of the fall which corresponded with a horizontal plane passing through my eye and the water. The sun was masked by cloud at the time.

I am not aware of any existing explanation of this class of optical phænomena, and I may be premature in venturing the following.

I conceive the effect to be owing to an involuntary and *unconscious* muscular movement of the eyeball, and thus occasioning a displacement of the images on the retina.

Supposing the eyes to be intently gazing at any point in a

* Communicated by the Author.

† The colour is brown from flowing over peat moors.

transverse plane passing through a vertically moving body, they will naturally and even irresistibly tend to follow the motion of that body; nor can the muscular apparatus of the eye maintain a stable equilibrium when the sight is fatigued and bewildered with a rapid change of moving forms before the eye.

Now in the case of the descending water, the eyes, being directed to a particular part in a horizontal section of it, cannot be prevented moving downwards through a small space: every new form in the moving scene invites the eyes to observe, and for that reason to follow it; but the voluntary powers are engaged to raise the axes of the eyes again to the section. This depression of the axes below the *intentional point of sight* seems to be repeated three or four times per second, whilst looking at the water-fall. Then, when the eyes are suddenly turned upon the rock, the muscles, having been brought into a kind of periodic contraction, will perform at least one of these movements after the exciting cause ceases to act; and thus the axes of the eyes, by moving downwards, will occasion a motion of the image of the rock over the retina in a direction from above downwards, and consequently the object giving that image will *appear* to move the contrary way, that is, upwards, agreeably to observation.

The deception, so far as I could judge, seemed to continue for a time equal to the interval of a periodic motion of the eye downwards when looking at the water, and, as before stated, one third or one fourth of a second.

The same kind of phænomenon may be produced by moving the eye before fixed bodies, and also when the motions are executed horizontally.

I have since been enabled to observe the appearance, with certain peculiar variations, whilst travelling parallel to one side of a narrow valley or lake, and looking across to the other. It takes place when moving in ships in sight of proximate land.

It is also producible by mechanical means, such as by a rapid unrolling of pieces of calico having some pattern or markings on them; and likewise by moving the head up and down, or laterally: but to particularize all the circumstances would make this communication inconveniently long.

LII. *An Account of some curious Facts respecting Vision. By* K.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I THINK the following facts, which, I believe, have not been before observed, may be sufficiently interesting to find a place in your Journal.

About six months since I suffered much from nervous headaches, which terminated in amaurosis of the left eye.

For some time after the commencement of the disease, I could perceive with the diseased eye, though with some difficulty, the letters forming the body of the Philosophical Magazine.

Most of your readersare probably aware that by an act of volition it is possible to double the image of an object viewed by both eyes. By means of this power I was enabled readily to transfer the letters of a page of your Magazine, seen by the left (the diseased) eye, to the right of the page, and to cause the bottom of a line of the printing seen with that eye, to coincide with the bottom of a line viewed by the right eye, and I could thus compare with considerable accuracy the relative dimensions of the letters as seen by the diseased eye, with those viewed by the sound eye.

To my great surprise I found the letters viewed by the diseased eye to be just one half the height of those seen by the right eye.

The size of the image formed on the retina must be in both eyes the same, or very nearly the same, the focus of the left eye being about nine, and that of the right eye ten inches.

From this experiment we seem necessarily led to the strange conclusion that it is not merely by the size of the image formed on the retina that we judge of the dimensions of objects, but that our perception of magnitude must be referred to some other source, and that this perception is modified by the *state* of the retina.

I have now lost the power of seeing any type smaller than the words " Philosophical Magazine " in your first page, and those words I should not be able to read did I not know them. It may be necessary to remark, that a very strong light is so far from assisting my vision, that in the light of the sun scarcely a trace of these letters is perceptible.

That type the letters of which are nearly half an inch high I can read, but it appears cloudy and indistinct. A page of

your Magazine has the appearance of a cloudy surface, the separation of the lines being scarcely distinguishable.

If now I employ convex lenses, I find that by using a lens of two inches' focus, I can read your Magazine with perfect ease, the letters appearing black and distinct.

I pretend not to offer any explanation of these phænomena, but as the facts may be relied upon, their statement may possibly tend to promote further inquiry on so interesting a subject. One practical advantage may be drawn from them, viz. that where the power of reading is unhappily lost by amaurosis of both eyes, it may possibly be restored by the use of a convex lens of about two inches' focus.

I may add, that with the diseased eye I am able to perceive the outline, though not the details, of the objects around me.

I am, Gentlemen, yours, &c.

London, Oct. 1834. K.

LIII. *Account of some Magneto-electrical Experiments made with the large Magnet at the Exhibition Room, Adelaide-street. By Mr.* Sturgeon.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

HAVING obtained permission of the proprietors of the Exhibition Room, Adelaide-street and Lowther Arcade, to employ their large magnet in any new experiments which I might wish to undertake, I availed myself of that privilege for the first time on Thursday evening the 28th of August last. The following results were produced.

The decomposition of hydriodate of potassa in solution: First, by paper moistened in it placed on a platinum plate attached to the negative side of the circuit; and with a platinum point connected with the positive side, the upper part of the paper was occasionally touched, whilst the magnet was at work. At each touch, however short the interval, iodine was evolved at the positive point.

Second: a solution of the hydriodate and starch was placed in a rectangular glass box, with a gauze partition to separate it into two compartments. A platinum plate, properly connected, was placed in each compartment. In half a minute the positive cell was completely obscured by liberated iodine. A more striking experiment was never exhibited. Platinum wires, properly connected in the circuit, were placed in solu-

tion of acetate of lead:—the metal was revived on the negative wire.

Solution of sulphate of copper was also subjected to the action of the current. Decomposition immediately took place, and the negative wire became completely covered with copper.

Water was also decomposed, the hydrogen and oxygen being collected in separate tubes.

The above experiments were made by changing the connexions and reversing the current; and the results were exhibited with as much promptitude as they could have been by the employment of a voltaic battery.

Hard steel was also magnetized by being placed in a helical part of the circuit. The poles were reversed at pleasure by changing the connexion.

A soft iron horse-shoe was magnetized to as high a degree as by a voltaic battery.

I have also made a great variety of electro-magnetic rotations, and some other rather novel motions, with electric currents by magnetic excitation, which I intend to publish as soon as opportunity permits.

By publishing the above in the forthcoming Number of your valuable Magazine you will much oblige

Your very obedient servant,

W. STURGEON.

Artillery Place, Woolwich,
Oct. 15, 1834.

P. S. I beg permission thus publicly to acknowledge the obligations under which I am placed for the very handsome manner with which Mr. Payne undertook to procure me the use of the magnet, and for the very able assistance of Mr. Maugham, Chemical Lecturer, whilst carrying on the experiments. W. S.

LIV. *Apparatus for Freezing Water by the Aid of Sulphuric Acid.* *By* R. HARE, *M.D., Professor of Chemistry in the University of Pennsylvania**.

THE congelation of water by its own vaporization, accelerated by exposure to the absorbing power of sulphuric acid, or other agents, *in vacuo*, has always been a difficult experiment. A distinguished Professor complained to me lately of want of success in his efforts to repeat it. In November 1832, after having three times succeeded in freezing water by the process in question, yet having failed before my class, I was ed to give more than usual attention to the

* Communicated by the Author.

process, in order to obviate the causes of disappointment. It appeared to me that the failure arose from imperfection in the vacuum. An excellent pump, with perfectly airtight cocks, is indispensable; and not only must the pump be well made, it must likewise be in good order. Neither should the packing of the pistions, the valves, nor the cocks, allow the slightest leakage. If a pump has been used previously for freezing by the evaporization of æther, it will not be competent for the experiment in question, unless it be taken apart and cleaned.

Cocks of the ordinary construction are rarely if ever perfectly airtight, and their imperfection always increases with wear. Under these impressions, having cleansed my air-pump, and put it into the best order possible; for the purpose of obviating leakage through the cocks associated with the instrument, I closed the hole in the centre of the air-pump plate by a screw, and for a receiver made use of a bell glass with a perforated neck, furnished with a brass cap and a female screw, by means of which one of my valve cocks was attached. A communication between the bell and the chambers of my pump was established through the valve cock and a flexible lead pipe, in a mode analogous to that already described in the account of the valve cock. In this way I succeeded in preserving the vacuum longer than when the cocks of the air-pump were employed in the process, and accomplished the congelation of water by means of the vacuum and sulphuric acid.

Latterly, I have used an apparatus in which a brass cover is made to close a large glass jar so as to be quite tight. In operating, the bottom of the jar was covered with sulphuric acid, and another jar with feet, also supplied with acid enough to make a stratum half an inch deep on the bottom, was introduced as represented. The bottom of the vessel last mentioned was, by means of the feet, kept at such a height above the surface of the acid in the outer jar as not to touch it. Upon the surface of the glass vessel, a small piece of very thin sheet brass was placed, made concave in the middle, so as to hold a small quantity of water.

The brass cover was furnished with three valve cocks, one communicating with the air-pump, another with a barometer gauge, and the third with a funnel supplied with water. Under these circumstances, having made a vacuum on a Saturday, I was enabled to freeze water situated on the brass, and to keep up the congelation till the Thursday following. As the water in the state of ice evaporates probably as fast as when liquid, during the night the whole quantity frozen

would have entirely disappeared, but for the assistance of a watchman, whom I engaged to supply water at intervals. At a maximum, I suppose the mass of ice was at times about two inches square, and from a quarter to a half an inch thick. The gradual introduction of the water, by aid of the funnel and valve cock, also of a pipe by which it was conducted to the cavity in the sheet brass, enabled me to accumulate a much larger mass than I could have procured otherwise. A brass band embracing the inner jar near the brim, with the three straps proceeding from it, serves to keep this jar in a proper position; that is, in fact, concentric with the outer jar.

In this last-mentioned experiment, I employed an air-pump upon a new construction, which I have lately contrived, and of which I shall soon publish a description.

LV. *Proceedings of Learned Societies.*

ZOOLOGICAL SOCIETY.

1834. July 8.—A LETTER was read addressed to the Secretary by M. Julien Desjardins, Corr. Memb. Z.S., dated Mauritius, January 10, 1834. It accompanied a collection of objects of Zoology, consisting chiefly of *Mammalia* and *Birds*, which were exhibited to the Meeting.

Mr. Gray exhibited various undescribed *Shells*, chiefly contained in his own collection. He characterized them as Unio *Novæ Hollandiæ*, and Anodon *Parishii*, *penicillatus*, and *porcifer*; the characters of each species being given in the Society's "Proceedings."

Mr. Gray also exhibited specimens of several *Shells*, which he referred to a genus to be separated from *Helix* under the name of Nanina. Helix (pars), *Fér.* Vitrina (pars,) *Quoy*. *Animal.* Collare amplum, lobo dextro antico, antro respirationis in sinu posito, lobo sinistro postico lato expanso partem inferiorem testæ anfractûs ultimi tegente. Pes posticè truncatus, processu brevi conico dorsali supra truncaturam sito. *Testa* depressa, perforata, polita; aperturâ lunatâ; peristomate tenui, edentulo, costâ internâ vel nullâ vel obsoletâ. *Indiæ, Chinæ, &c. Incolæ.*

The shells comprised in this genus have been referred by M. De Férussac, and by most authors, to *Helix*: they are, however, more nearly related to *Vitrina*, with which M. Quoy intends placing them. But from the shell of *Vitrina* that of *Nanina* differs by being umbilicated, as well as by its smaller mouth. The lobation of the collar of the animal of *Nanina* distinguishes it also from *Vitrina*; the collar of the latter being entire, with a linear lobe on the side extending over the shell, and with the respiratory hole placed at its base.

The animal was first observed and figured by General Hardwicke in 1797.

The following species belong to the genus:

Nan. Nemorensis. Helix Nemorensis, *Müll.*

Nan. Javanensis. Hel. Javanensis, *Fér.*

Nan. exilis. Hel. exilis, *Müll.*

Nan. citrina. Hel. citrina, *Linn.*

Var. Hel. castanea, *Müll.*

Hel. Rapa, *Chemn.*

Nan. monozonalis. Hel. monozonalis, *Lam.*

Nan. Clairvillia. Hel. Clairvillia, *Fér.*

Nan. Vitrinoides. Hel. Vitrinoides, *Desh.*

NANINA JULIANA. *Nan. testâ solidâ, albâ; spirâ convexiusculâ; anfractibus depressis fasciâ medianâ brunneâ, ultimo anticè roseo fasciâ brunneâ axin cingente; peristomate rotundato, roseo.*

Axis 11, diam. 20 lin.

Hab. in Ceylon.

This is one of the most beautiful of the genus. It approaches to *Nan. Javanensis,* but is thicker and larger.

NANINA STRIATA. *Nan. testâ solidiusculâ, subpellucidâ, albidâ; periostracâ tenui, olivaceâ; spirâ convexiusculâ, confertim transversè striatâ; anfractu ultimo anticè sublævi.*

Axis 9, diam. 15 lin.

Mr. Gray also exhibited an extensive series of *Shells* of the Genus TEREBRA, forming part of his own collection, and illustrating an account of many new species of that group which he presented.

He stated that the animal has a small foot, and a very long *proboscis,* at the base of which are seated two very small *tentacula*; the *operculum* is ovate, thin, horny, rounded behind, and rather tapering in front. The shell is covered by a very thin, pellucid, horn-coloured *periostraca*: it is usually white, variously streaked with brown, the streaks being often interrupted or broken into spots by the two spiral bands of the shell; one of these bands is placed near the spiral groove and the other on the middle of the whorl. The *apex* of the cavity is frequently filled up by a calcareous deposition; but this deposition has never been observed in *Ter. duplicata.*

The species may be divided into the following sections:

I. *Anfractibus sulco spirali cingulum posterius efformante; labio interiore tenui, concavo.*

OBS. Cingulum in junioribus magis conspicuum; labium internum in adultis rarissimè incrassatum.

Huic sectioni referendæ sunt

Ter. maculata, Lam.

Ter. tigrina.—Buccinum felinum, *Dillw.*

Ter. strigata, Sow.—Buccinum elongatum, *Wood, Suppl., f.* 22.

Ter. dimidiata, Lam.

Ter. striatula, Lam.

Ter. flammea, Lam.

Ter. muscaria, Lam.

Ter. subulata, Lam.

Ter. oculata, Lam.
Ter. crenulata, Lam.
Ter. corrugata, Lam.
Ter. duplicata, Lam.
Ter. pertusa, Sow. Born, Mus., t. 10. f. 13.
Ter. nubeculata, Sow.
Ter. myuros, Lam.

The following new species also belong to this section, Ter. *Knorrii* (differing from *Ter. maculata* by being more slender, and by having the front of the whorls spotted; and from *Ter. tigrina* by the marbling of the back of the whorls), *affinis* (allied to *Ter. nubeculata*, but smaller and more slender in its proportions), *rudis*, *striata* (resembles *Ter. affinis*, but the grooves not punctate), *undulata*, *alba*, *flava*, *punctatostriata*, *gracilis*, *tessellata* (differs from all the other spotted species by the hinder belt being destitute of spots), *variegata*, *plicata*, *punctata*, *lævigata*, and *lævis*.

II. *Anfractibus sulco spirali cingulum posterius efformante; labio interiore incrassato subelevato.*

Obs. Quoad aperturam *Cerithia* quodammodo simulantes.

Huic sectioni referendæ sunt

Ter. cerithina, Lam.

Ter. tricolor, Sow.—*Ter. tæniolata*, Quoy, cui proprii sunt insuper sulcum cingulum efformantem sulci alii spirales duo.

Also the following new species: Ter. *anomala*, *ornata*, *cancellata*, *straminea*, and *triseriata*.

III. *Anfractibus sulco postico nullo.*

* *Labio interiore tenui.*

a. *Testâ elongatâ, gracili.*

Ter. lanceolata, Lam.
Ter. strigillata, Lam.
Ter. hastata, Lam.—Ter. costata, *Mœnch.*

And a new species, Ter. *albida*.

b. *Testâ brevi.*

Ter. aciculata.—Buccinum aciculatum, *Lam.*
Ter. polita.—Buccinum politum, *Lam.*

** *Labio interiore incrassato, elevato; testâ brevi.*

Obs. *Nassæ* quodammodo affines; sed neque labium internum dilatatum, nec externum incrassatum.

Ter. lineolata, Sow. Wood, Suppl., f. 22.

Ter. Tahitensis.—Buccinum Tahitense, *Gmel.*—Buccinum Australe, *Sow.*

Mr. Gray concluded by stating that specimens of all the species of *Terebra* enumerated by him are contained either in his private collection or in the British Museum.

Mr. Gray also exhibited an extensive series of *land* and *freshwater Shells* which he regarded as hitherto undescribed. He characterized them as Helicophanta *Falconeri*, Reeve, MSS., Zonites *Walkeri*, and Bulimus *atomatus*.

The three following species were discovered in the interior of New Holland by Mr. Allan Cunningham, and two of them have

been figured, but not described, in Mr. Griffith's Edition of Cuvier's 'Animal Kingdom.' viz. Helix *Cunninghami,* Gray, in Griff. Anim. Kingd., t. 6. f. 4.; *Fraseri,* Gray, in Griff. Anim. Kingd., t. 6. f. 6.; and *Jacksoniensis* (resembles *Hel. nitida* in form, but is imperforate).

To Mr. Cunningham Mr. Gray was also indebted for three species discovered by him in Phillip's Island, a small island about 5 miles South of Norfolk Island. These he characterized as Hel. *Campbellii,* and *Phillipii*—(this species is allied to the former in the shape of the mouth and structure of the lip; but the whorls are angular in the young state only, as in most of the *Helices* of Lamarck,)—and Carocolla *Stoddartii.*

The remaining species were described from specimens in Mr. Gray's own collection; they were characterized as Bulimus *rhodostomus, crassilabris, apiculatus* (resembles *Bul. Kingii,* but is more solid and has a dark *apex* and pillar), *Pullus,* and *Burchellii*; Lignus *tenuis,* Helix *Codringtonii, fidelis, Cracherodii* (perhaps a *Nanina,* but is more largely perforated than any of that genus of which Mr. Gray has seen the animal), and *Maderaspatana.*

While on the subject of Indian *Helices,* Mr. Gray remarked that *Hel. ligulata,* Fér., Moll., t. 31. f. 2, 3, is a common Indian species; and that *Hel. cicatricosa,* Chemn., vol. ix. t. 109. f. 913, is found in the more elevated regions of India, and has lately been described by Mr. Lea under the name of *Hel. Himalayana.*

Also Carocolla *Novæ Hollandiæ,* Helix *granifera* and *pachygastra.*

Mr. Gray observed, when characterizing the last-named shell, that he calls that a tooth which is solid, and that a plait which is marked externally by a corresponding groove. Thus the *Chondri* of Cuvier have toothed mouths, and the *Pupæ* and *Clausiliæ* plaited.

The exhibition was resumed of the new species of *Shells* contained in the collection formed by Mr. Cuming, chiefly on the Western Coast of South America and among the islands of the South Pacific Ocean. Those brought on the present occasion under the notice of the Society were accompanied by observations and characters by Mr. G. B. Sowerby, and comprised the following species of the genus Pholas, the characters of which are given in the Proceedings.

"The utmost caution is necessary in the examination and description of the various sorts of *Pholades,* on account of the extraordinary difference in the form of the same species in different stages of growth. The addition of accessory valves also, as they increase in age, must be carefully observed, in order to guard against too implicit a confidence in their number and form. And though I might be considered guilty of asserting a truism by stating that the difference in size of different individuals of the same species may and sometimes does mislead the tyro in the science of Malacology; lest such difference should mislead the adept also, let him too proceed cautiously, and when he finds a fully grown shell of half an inch in length agreeing perfectly in proportions and characters with another of two inches long, let him not conclude that it is a distinct

species, but if he can find no other difference except that which exists in their dimensions, let him consider the one a giant, the other a dwarf. Let it be remembered that among the *Cypræa* it is not uncommon to observe young shells of three inches in length, and fully grown ones of the same sort only one inch in length; likewise, of the well-known British *Pholades* there are individuals quite in a young state of two inches in length, and perfectly formed shells of the same species not more than half an inch long. For an instance in demonstration I need only refer to the *Phol. papyraceus*, so abundant at Torquay, of which the young shells have been considered by many as a distinct species and have been named by Dr. Turton *Phol. lamellosus*. This varies in size exceedingly, so that it may be obtained both in an incomplete and young state and in a fully grown condition from half an inch to nearly two inches in length. The circumstance of its having rarely occurred in an intermediate state of growth, when the anterior opening is only partly closed and the accessory valves only partly formed, led Dr. Turton and others to persist in regarding the young and old as two distinct species. Other similar instances will be shown in the course of the present concise account of some hitherto undescribed species of the same genus brought to England by Mr. Cuming."—G. B. S.

Pholas *cruciger*, *Chiloensis*, var. parva, *subtruncata* (very like our British *Pholas parva*), *calva*, Gray, MSS., *calva*, var. nana, *acuminata*.

One specimen of the last-named shell in Mr. Cuming's collection merits particular notice. It demonstrates a fact of considerable importance to geologists. It is in argillaceous limestone, very much resembling lias, and in forming the cavity in which it resides, it has, by such chemical process as frequently takes place, absorbed a much greater quantity of the rock than could be retained or converted; this is again deposited at the upper part of the cavity; and thus the rock is recomposed.—G. B. S.

Phol. *melanura*, *tubifera* (resembling in a marked manner the *Pholas papyracea* of Southern Devonshire), *Quadra*, *Quadra*, var. (with the *epidermis* which covered the muscle contained in the concave reflected anterior dorsal margin changed into calcareous matter), *curta*, and *cornea*.

The whole of the *Toucans* of the Society's collection were exhibited in illustration of an account given by Mr. Gould, at the request of the Chairman, of the species of *Ramphastos*, Ill., and *Pteroglossus*, Ej., constituting the family *Ramphastidæ*. Mr. Gould's attention having been of late particularly directed to this family in the preparation of a Monograph of it, illustrated by coloured figures of all the birds comprised in it, he was enabled to state the existence of the under-mentioned species of the Fam. Ramphastidæ, *Vig*. *Rostrum* magnum, ad basin nudum; *tomiis* serratis. *Lingua* pectinata. *Pedes* scansorii.

Genus Ramphastos, *Ill.* Ramphastos (pars), *Linn*. *Rostrum* maximum. *Nares* frontales, prope basin maxillæ sitæ. *Cauda* æqualis.

Nigri, torque pectorali tectricibusque caudæ inferioribus coccineis, pe-

dibus cœruleis. Rostrum, guttur, tectrices caudæ superiores, orbitæque nudæ discolores.

* *Caudæ tectricibus superioribus flavis.*

Ramph. *erythrorhynchus*, Gmel., *Cuvieri*, Wagl., *culminatus*, Gould.

** *Caudæ tectricibus superioribus albis.*

Ramph. *Swainsonii*, Gould, *carinatus*, Swains., *Toco*, Gmel.

*** *Caudæ tectricibus superioribus coccineis.*

Ramph. *vitellinus*, Ill., *Ariel*, Vig., *dicolorus*, Linn.

Genus Pteroglossus, *Ill. Rostrum* magnum. *Nares* superæ, in maxillæ basi sitæ. *Cauda* gradata.

Suprà viridescentes, uropygio (nisi in perpaucis) discolore; subtùs, capite, collo, rostro, orbitisque nudis utplurimùm discoloribus; pedes cœrulei.

Pter. *Aracari*, Ill., *regalis*, Licht., *castanotis*, Gould, *bitorquatus*, Vig., *Azaræ*, Wagl., *ulocomus*, Gould, *hypoglaucus*, Gould, *Bailloni*, Wagl., *viridis*, Ill., *inscriptus*, Swains., *maculirostris*, Licht., *Culik*, Wagl., *prasinus*, Licht., and *sulcatus*, Swains.

The whole of the species characterized above are figured in Mr. Gould's 'Monograph of the Ramphastidæ,' which is just completed; and all of them, with the exception of *Pteroglossus Azaræ*, *Pter. inscriptus*, and *Pter. prasinus*, are contained in the Society's collection, and were exhibited to the Meeting.

July 22.—A letter was read, addressed to Mr. Vigors by B. H. Hodgson, Esq., Corr. Memb. Z.S., and dated Nepâl Residency, February 14, 1834. It referred to various living animals which it is the intention of the writer to forward to Calcutta for transmission to England during the ensuing season. It also referred to a collection of skins of *Mammalia* and *Birds* which have already been dispatched by Mr. Hodgson for the Society. Among them are skins of the *Chiru Antelope*, *Antilope Hodgsonii*, Abel, male and female; and the writer refers to these as elucidating the points which had been unascertained by him at the time of making to the Society his several previous communications, abstracts of which have been published in the Proceedings of the Committee of Science and Correspondence, Part i. p. 52, and Part ii. p. 14; and in the Proceedings of the Society, Part i. p. 110. An abstract of part of the letter is given in the Proceedings, No. xix.

Some extracts were read from a Letter addressed by the President, Lord Stanley, to the Secretary, giving an account of the breeding of several *Birds* in His Lordship's Menagerie at Knowsley. The *red Grosbeak*, *Loxia Cardinalis*, Linn., has a nest of three young which are nearly fledged; and a single young one of the *Towhee Bunting*, *Emberiza erythrophthalma*, Gmel., has been hatched. The *Loxia cucullata* has this year, as last year also, made a nest and laid one egg; and the *American yellow Bird*, *Fringilla tristis*, Linn., is now sitting.

The gosling of the *Sandwich Island Goose*, respecting which a notice from Lord Stanley was read on May 27, "is now fully as large as the parents, and nearly resembles them in plumage; the only differences being about the neck, which is more

indistinct in front and wants the full extension of the black down the nape, and the collar at the bottom just above the breast is only faintly marked. The legs also are as yet of a dirty greenish yellow tinge. It is not pinioned, but has hitherto shown no wish to use its wings. In fact they are the tamest of the tame, scarcely will move out of one's way if in the walks, and are constantly coming into the building, even more familiarly than the *common Ducks.*"

A specimen was exhibited of the *Manis Temminckii*, Smuts, forming part of the collection made by Mr. Steedman in Southern Africa. Mr. Bennett stated that his object in calling the attention of the Society to it was to point out the external characteristics of a species known to its original describer by its skeleton alone and by a few detached scales.

It may be thus characterized:

Manis Temminckii, Smuts. *Man. capite breviore; corpore latiore, squamis magnis, 11-seriatis; caudâ truncum longitudine subæquante, latitudine paullò minore, ad apicem subtruncatum vix angustiore.*

Hab. apud Latakoo?

Long. tot. $25\frac{1}{2}$ unc.; *caudæ*, 12; lat. *dorsi*, 8; *caudæ*, prope apicem, 5.

The most remarkable features of this animal are the shortness of the head; the breadth of the body; and the breadth of the tail, which is nearly equal to that of the body, and continues throughout the greater part of its extent of nearly the same width, tapering only slightly towards the end where it is rounded, and almost truncate. In the shortness of the head and the general form of its upper part, the *Man. Temminckii* bears nearly the same relation to the *Man. Javanica*, as is borne by the *Weasel-headed Armadillo, Dasypus 9-cinctus*, Linn., to the *six-banded, Das. 6-cinctus*, Ej. Of the eleven series of scales on the body, one on each side is ventral rather than dorsal. The scales are very large, longitudinally striate, smooth as though rubbed towards their hinder margin, and slightly produced into a thin, short, and rounded process: they are comparatively few in number, the large scales of the middle line of the back from the *occiput* to the tip of the tail being twenty only in number; in *Man. pentadactyla*, Linn., they are about thirty; and in *Man. Javanica*, Desm., they vary from about forty-five to fifty. A peculiarity in the distribution of the scales of *Man. Temminckii* is the cessation of the middle series of them at a short distance anterior to the extremity of the tail, so that the last four transverse rows consist of four scales each, each of the preceding ones having five.

Some notes by Mr. Rymer Jones of the dissection of an *Agouti, Dasyprocta Aguti*, Ill., were read, and are given in the Proceedings.

The animal was a male; adult; measuring $19\frac{6}{10}$th inches from the extremity of the jaws to the root of the tail; and weighing 4lbs. $4\frac{1}{4}$oz. Its head measured $4\frac{6}{10}$ inches in length; the tail, $1\frac{9}{10}$.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The following is an outline of the proceedings at the late meeting of the British Association, holden at Edinburgh from the 8th to the 13th of September, Sir Thomas Makdougall Brisbane, K.C.B., F.R.S., &c., &c., President, giving the titles of the papers and other communications which were read or received.

Section of Mathematics and General Physics.
Chairman.—Rev. W. Whewell.

A report by Mr. Challis on the theory of Capillary Attraction.—On the Repulsion produced by Heat, as established by the contraction of Newton's rings when heat was applied to the glasses, by Prof. Powell.—Mr. Whewell read a letter from Mr. Hailstone, accompanying a table of Barometrical Observations, taken at short intervals.—Prof. Forbes read a short communication from Mr. Christie, on a remarkable Meteorological Phænomenon observed by him at Woolwich.—Prof. Lloyd read to the Section a portion of his report on Physical Optics.—A paper by Mr. Challis, entitled, Theoretical Explanations of some facts relating to the composition of the Colours of the Spectrum.—A paper by Prof. Powell, On the Achromatism of the Eye.—Prof. Phillips read the second Report of the result of twelve months' experiments on the quantity of rain falling at different elevations above the ground, made by himself and Mr. Gray.—A paper by Prof. Stevelly, entitled, An attempt to connect some well known phænomena in Meteorology with well established physical principles.—The second part of a Report on Hydraulics, containing the application of the principles of that science to the subject of rivers, by Mr. Rennie.—On a new method in Dynamics, by Prof. Hamilton.—On a new formof the Dipping Needle, constructed so as to afford the means of correcting the error of the centre of gravity, by Prof. Phillips.—Notes on the mean temperature in India, by Col. Sykes.—On Magnetical Observations undertaken in Ireland, by Prof. Lloyd.—Sir David Brewster described to the Meeting a remarkable coloration which he had observed in the space included between the interior and exterior rainbow.—M. Saumarez read a paper on Light and Colours, containing some peculiar views respecting their nature and origin.—Mr. Ramage read a proposal for constructing a reflecting telescope of greater magnitude than has yet been attempted.—Dr. Knight exhibited to the Section a method of rendering the vibrations of heated metals perceptible to the eye.—Mr. Russell gave an account of some recent experiments on the traction of boats on canals at great velocities.—Sir David Brewster communicated to the Section the results of a series of experiments on the effects of reflection from the surfaces of crystals, when some surfaces have been altered by solution; and exhibited a number of singular forms, produced by different crystals, or by the same crystal under different circumstances.—Mr. Graves presented a paper on the theory of Exponential Functions, in further illustration of a memoir on the same subject which he had laid before the Royal

Society, and which had been printed in its Transactions.—Prof. Hamilton explained a new method of conceiving imaginary quantities, and the principles of a theory which he denominated "the theory of conjugate functions." Prof. Hamilton stated, that he had confirmed, by the aid of the theory, the results obtained by Mr. Graves.—Mr. Sang stated the results of some theoretical and experimental investigations which he had made on the nature of those curves traced by the extremities of vibrating wires fixed at the end, and he exhibited drawings of the forms of the curves thus produced. —On the Production and Propagation of Sound, by Dr. Williams.—On the Visibility of the Moon in total Eclipses, by Dr. Robinson.—On Collision, by Mr. Hodgkinson.

Section of Chemistry and Mineralogy.
Chairman.—Dr. Hope.

Mr. Johnston and Mr. Harcourt gave an account of the state of the experiments they have respectively undertaken, on the comparative analysis of Iron in the different stages of its manufacture, and on the effects of long-continued heat.—Prof. Whewell made a communication from the Committee appointed to examine the subject of Isomorphism.—A paper was read by Dr. Charles Williams, On a new law of Combustion, and the production of Flame at a low temperature.—On the employment of coal-tar in connexion with water as fuel, by Prof. Daubeny.—On the discoveries of Reichenbach in regard to the products of the destructive distillation of organic substances, by Dr. Gregory*.—A notice of a large specimen of amber from Ava, intersected by thin layers of carbonate of lime, by Sir David Brewster.—Mr. Van der Toorn gave a determination of the amount of water in crystallized sulphate of zinc. The total amounts to 7 atoms, of which 6 are given off at 110° C., the other atom remaining as a necessary constituent of the salt. From this result he concluded that sulphates, which at a red heat give off sulphuric acid, contain an atom of water as an essential constituent.—On the amount of carbonic acid in the atmosphere of the town of Bolton, and the country around, by Mr. Watson.—On the present state of our knowledge regarding contagion, by Dr. W. Henry.—An analysis of the oxichloride of antimony or crystallised powder of Algaroth, by Mr. Johnston.—The Rev. W. V. Harcourt described the objects of the experiments now in progress under his superintendence, for determining the effect of long-continued heat on various mineral substances, and the various methods adopted by him in disposing them beneath the iron furnaces of Yorkshire.—Dr. Clark gave an account of Mr. Nixon's process for smelting iron by the aid of the hot-blast, and exhibited numerical results of the advantages derived from the new process. The saving is so great, that the total amount of coal now necessary to produce one ton of iron amounts only to 2 tons 14 cwt.; whereas formerly it required 8 tons 1½ cwt., being a saving of 5 tons 8 cwt. for each ton of iron produced. This subject was discussed at considerable length.—

* See Lond. and Edinb. Phil. Mag. vol. i. p. 402; and vol. iv. p. 390.

On the Optical characters of Minerals, by Sir David Brewster.—An account of an investigation of the constitution of certain hydrated salts, by Mr. Graham.—On a new mode of liquefying the gases, by Mr. Kemp.—On the electro-magnetic condition of mineral veins, by Mr. Fox.—On the dimorphism of the sesqui-iodide of antimony, by Prof. Johnston.

Section of Geology and Geography.

Chairman.—PROF. JAMESON.

The Section entered upon a discussion of the views of Dr. Boase relative to stratification, &c., arising out of the last year's proceedings.—Dr. Rogers's Report on the Geology of North America was read, illustrated by maps.—A Report by Mr. Stevenson, On the state of our knowledge respecting the relative level of land and sea, and the waste and extension of land on the east coast of England, illustrated by charts and sections of the German Ocean.—Lord Greenock read a paper on the coal formation of the central district of Scotland.—A notice by Mr. Menteath on the Closeburn limestone was read, in which an account was given of the geological, mineralogical, and chemical characters of that deposit.—A notice was read by Mr. Trevelyan, on fossil wood from a bed of clay lying above coal in Suderoe, the most northern of the Faroe Islands.—Dr. Hibbert read a paper, On the ossiferous beds contained in the basins of the Forth, the Clyde, and the Tay. A large collection of fossils connected with this paper were submitted to the inspection of M. Agassiz, and became the subjects of highly important investigation, and communications from him during the sittings of the Association.—On the structure of recent and fossil Woods, by Mr. Nicol.—Some remarks on the Geology of the Orkneys, by Prof. Traill.—On the Geology of Berwickshire, by Mr. Milne.—Notice of some caverns containing bones near the Giant's Causeway, by Mr. J. Bryce.—A general view of the relation of joints and veins, by Prof. Phillips.—A tabular view of the order of succession of various formations of great thickness, and distinct from each other in their organic remains and mineralogical characters, which rise from beneath the old red sandstone of England and Wales, by R. I. Murchison, Esq.—M. Agassiz delivered some highly interesting observations on the fossil fishes of Scotland.—On the flints found in various parts of Aberdeenshire, and more especially in the vicinity of Peterhead, by Dr. Knight.

Section of Natural History.

Chairman.—PROF. GRAHAM.

A report was read on the recent and present state of Zoology, by the Rev. Leonard Jenyns, F.L.S.—Also, An account of excursions in the neighbourhood of Quito, and towards the summits of Chimborazo and Pichincha, by Colonel Hall.—Prof. Agassiz next made a highly valuable communication upon the different species of the genus Salmo which frequent the various rivers and lakes of Europe, in which he reduced the species to six.—On the plurality and development of embryos in the seeds of Coniferæ, by Robert Brown,

V.P.L.S.—On the functions and use of the orbital glands of Birds of the orders Natatores and Grallatores, by P. J. Selby, Esq.—On the Birds observed and collected during an excursion in Sutherlandshire, by P. J. Selby.—On the Fishes obtained during the same excursion, by Sir W. Jardine.—On the Insects obtained, by James Wilson, Esq.—On a collection of Insects recently received from Java, by James Wilson, Esq.—On the change of colour of the fruit in a certain species of Elder, by the Rev. James Drake.—On the cultivation of *Phormium tenax* in Scotland, by John Murray, Esq. —On the progress made in researches on the secretions from the roots of vegetables, by Dr. Dunbar.—On the distribution of the Phenogamous plants of the Faroe Islands, by W. C. Trevelyan, Esq. —A memoir on the propagation of Scottish Zoophytes, by Mr. Dalzell.—Account of the Natural History of the central portion of the great mountain range of the South of Scotland, in which arise the sources of the Tweed, by W. Macgillivray, Esq.—On the *Coculus indicus* of commerce, by G. Walker Arnott, Esq.—On the head of *Delphinus Deductor,* on the laryngeal sac of the Reindeer, and on a new species of Thrush from Nepaul, by Dr. Traill.—On the transformations of the Crustacea, by J. O. Westwood, Esq.—On a peculiar race of Men supposed to have constituted the inhabitants of the elevated regions, situated between the 14th and 19th degrees of south lat., in South America, by Mr. Pentland.—On some peculiar secretions and elaborations, viewed in connexion with the ascent of the sap, by John Murray, Esq.—On a new species of Pecten, by T. Brown, Esq.—On the progress of successive vegetation, at various heights, on the Himalaya Mountains, by J. F. Royle, Esq.—Some observations on the structure of Feathers, by Sir David Brewster.

Section of Anatomy and Medicine.

Chairman—Dr. Abercrombie.

Mr. Broughton read to the Section the results of an experimental inquiry respecting the sensibilities of the nerves of the brain.—Dr. Alison read a notice of some experiments by Dr. J. Reid, illustrating the connexion of the irritability of muscles with the nervous system, with observations by himself.—On certain peculiarities in the circulation of the Porpoise, illustrated with preparations, by Dr. Sharpey.—A communication from Mr. Murray of Hull on the change of colour in the Chameleon.—An abstract of a registry kept in the Lying-in Hospital of Great Britain-street, Dublin, from the year 1758 to the end of 1833, and which illustrated the importance of thorough ventilation in such establishments, by Dr. Joseph Clarke of Dublin. The author died in Edinburgh during the meeting of the Association.—Dr. W. Thomson read a paper on the infiltration of the lungs with black matter, and on black expectoration, occurring in coal-miners, iron-moulders, &c.—Sir C. Bell delivered a discourse explanatory of his views of the functions of the nervous system, and of the manner in which this department of physiology should be studied.—On the varieties of mechanism by which the blood may be accelerated or retarded in the arterial and venous systems of the Mammalia, by Dr. Aitkin.—Dr. Hodgkin read a re-

port on the history of the results of the experimental inquiry respecting the action of poisons, the prosecution of which was committed by the Association at its last meeting to himself and Dr. Roupell.—Report on the present state of physiological science, by Prof. Clarke.

The last Meeting of the Association was held in the large and splendid hall of the college library, the galleries of which were set apart for the accommodation of ladies. The doors were thrown open about half-past two o'clock, when a great rush was made for admission; and a little after three o'clock, when the business commenced, the hall was filled. A short time before this the Lord High Chancellor Brougham made his appearance on the platform.

It was announced to the Meeting by the President, Sir Thomas Makdougal Brisbane, that invitations to the British Association had been received from the Bristol Institution, the Literary and Philosophical Society of Liverpool, the Royal Dublin Society, the Royal Irish Academy, the Geological Society of Dublin, and the University of Dublin. At the final Meeting it was announced that the General Committee had unanimously resolved that the invitations of the constituted scientific authorities in Dublin should be accepted; and that the next Meeting of the Association should be held in Dublin, on Monday 10th August, 1835; and that the thanks of the Association had been voted to the Bristol Institution, and to the Literary and Philosophical Society of Liverpool, from whom invitations had also been received; and the Rev. Vernon Harcourt, General Secretary, stated the results of the proceedings of the General Committee on the subjects brought before them for consideration by the Sectional Committees, as to grants of money, requests for reports on the progress of science, and recommendations of special subjects of scientific inquiry. They had authorized the appropriation of part of the funds of the Association for the purpose of prosecuting particular researches in physical, chemical, geological, zoological, botanical, and medical science, to the extent of 830*l.* They had authorized the application for the continuation of Reports on various branches of science to Rev. G. Peacock, Rev. J. Challis, Rev. R. Willis, Mr. George Rennie, Prof. Rogers, and Mr. Stevenson; for a Report on the application of mathematical science to the phænomena of heat, electricity, and magnetism; on electro-chemistry and electro-magnetism, to Dr. Roget; on the zoology of North America, to Dr. Richardson; on the botany of North America, to Prof. Hooker; on the geographical distribution of plants, to Prof. Henslow; on the geographical distribution of insects, to Mr. J. Wilson; on the pathology of the nervous system, to Dr. W. Charles Henry; and on the effect of circumstances of vegetation on the medicinal virtues of plants, to Dr. Christison.

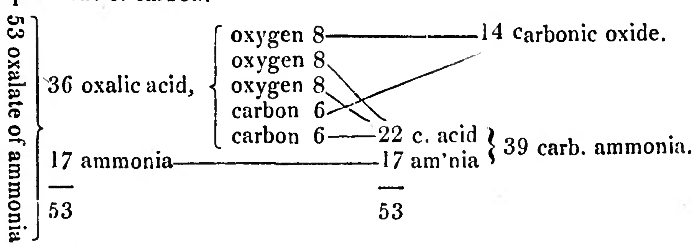
Various recommendations of special subjects for inquiry were sanctioned by the General Committee, and ordered to be printed in the next volume of the publication of the Association.

LVI. *Intelligence and Miscellaneous Articles.*

OXIDE OF CARBON FREE FROM CARBONIC ACID.

DR. MITCHELL states that he has obtained oxide of carbon of excellent quality, independently of the use of lime water or any other agent for the purpose of detaching carbonic acid, by the action of sulphuric acid on the oxalate of ammonia. The process is as follows: Take an ounce of the oxalate, reduced to powder, and a drachm or two of sulphuric acid, and put them into a six-ounce tubulated retort, and apply a very gentle heat. In a few minutes large quantities of gas are evolved, and may be collected in the usual manner over water. If the heat be duly moderated, the first and last products, as obtained in the receivers, will be pure carbonic oxide gas. The sulphuric acid seems to act by resolving the oxalate into oxalic acid and ammonia; then to decompose the oxalic acid into its elements, and to put the whole into such a state as to enable the constituents to recombine so as to form the pure gas. That carbonic acid is actually evolved, cannot be doubted, but it seems to join the ammonia instantly, forming carbonate of ammonia, which is absorbed by the water as fast as it is produced. If it is inquired how it happens that the sulphuric acid does not instantly seize the ammonia and form a sulphate, Dr. M. observes, that although the moderate heat employed is amply sufficient to drive over the gaseous elements of the oxalate, it is inadequate to cause the sulphuric acid to do so.

The above statement will be better understood by the use of a diagram; premising, that the equivalents or combining numbers of the several articles are as follow: *oxalic acid* 36, made up of 24, or 3 equivalents of oxygen, and 12, or 2 equivalents of carbon; *ammonia* 17, making the salt 53; *carbonic acid* 22, made up of 16, or 2 equivalents of oxygen, and 6, or 1 equivalent of carbon; *carbonic oxide* 14, composed of 8, or 1 equivalent of oxygen, and 6, or 1 equivalent of carbon.



If a very gentle heat be continued for some time, the same products will be had, independently of the use of sulphuric acid; but the latter seems to accelerate the process.

When we employ oxalic acid to make the carbonic oxide gas, a portion of carbonic acid is unavoidably formed, and must be removed by means of lime water. In like manner, this acid gas is generated or evolved when the oxalate of ammonia is used; but as it combines instantly with the ammonia, it does not contaminate the desired pro-

duct. A small portion of the carbonate of ammonia will be found along the beak of the retort, but for the most part, it is taken up by the water. The addition of a few drops of a solution of sulphate of copper to the fluid, strikes a blue colour instantly, thus denoting the presence of ammonia. On examining the residuary matter in the retort, it is found to be strong sulphuric acid." Dr. M. concludes with observing, "I know of no other rationale of this process, and think it quite satisfactory. Of one thing, however, I am certain, and that is, that no other method that I have employed, yields the gas in question, so pure, and with so little trouble. It is therefore confidently recommended to all operators in chemistry."—*Silliman's Journal*, vol. xxv. p. 344.

ANALYSIS OF THE BRAIN.

According to M. Couerbe, the brain, when examined with a powerful microscope, appears to be composed of globules which are slightly elliptical, and are larger in the grey substance than in the white. These globules are coagulable by acids, like those of milk and the blood, and by a great number of other substances.

M. Couerbe finds in the brain:

1st. A pulverulent yellow fat,	*stéaroconote.*
2nd. An elastic yellow fat,	*cérancéphalote.*
3rd. A reddish yellow oil,	*éléancephol.*
4th. A white fatty matter,	*cérebrote.*
5th. Cholestrine,	*cholestérote.*

Added to these are the salts found by Vauquelin, lactic acid, sulphur, and phosphorus, which form a part of the fats above named.

Before the brain was submitted to various kinds of treatment, it was deprived of its membranous covering and washed with cold water, in order to separate, as nearly as possible, all the blood with which it is always impregnated: it was then malaxated and digested in cold æther, and all that was soluble in this fluid was dissolved by maceration in repeated portions of it. The first contained but little of the fatty matter in solution; the æther appeared merely to expel the moisture of the brain, and they were separated together by decantation. The second portion of æther was very rich in fatty matter, and contained but slight traces of moisture: four macerations in æther are almost always sufficient to dissolve all the fatty portions of the brain. After treatment with æther, the brain was subjected to the action of boiling alcohol of sp. gr. 0·817: the boiling solutions were filtered every time, and the boiling was repeated until they gave no precipitate on cooling; there then remained a mere agglomerated fibrous mass, which M. Couerbe calls *névriline.*

The alcoholic solutions were mixed when cold, and filtered to separate the deposit, which was washed with cold æther, in order to separate the fat soluble in this liquid: this is susceptible of crystallizing, and perfectly similar to that which is found in the æthereal solution, and which is *cholestérote.*

The powder obtained from the alcohol is very white and pure: it becomes slightly translucid by drying, and has then the appearance of

purified wax. The alcohol from which this white powder precipitated, gave more of it by evaporation, mixed with some fatty matter which was separated by æther. The substance dissolved by the alcohol appears to be similar to that described by Vauquelin. M. Couerbe calls it *cérébrote.*

Towards the end of the evaporation of the alcohol, a sort of fluid fat is deposited, which is not the white fatty matter; it dissolves in æther, and is converted into oil during the spontaneous evaporation of that fluid. The alcoholic residue contains only osmazome, a free acid, and some inorganic salts.

The æthereal solution was distilled, in order to obtain the æther as well as the substances which it had dissolved. These were put into a capsule, in order to finish the expulsion of the æther. The fatty matters obtained were in considerable quantity, and in the state of a whitish homogeneous adhesive mass, under which there was frequently whitish granular fatty matter, almost entirely formed of cérébrote. This appearance was constant in the brains of healthy persons. This fatty matter was then treated with a small quantity of æther, which dissolved it entirely when free from the whitish granular fatty matter, but only partially when that was present.

This cérébrote is always found in the mass, distinct from other elements which accompany it when extracted from healthy persons; but, on the contrary, sufficiently combined with them to become soluble in a small proportion of æther, when taken from the brain of a maniac.

When, then, æther leaves any white substance, it is separated by the filter, and when the æther dissolves it entirely, it is to be evaporated to obtain more of the substance; the residue is to be subjected to the action of boiling alcohol, which dissolves the three fatty matters, among which is the cérébrote, and leaves undissolved a yellow solid fat resembling wax. This substance is almost totally insoluble in alcohol: it is to be washed several times with boiling alcohol to separate extraneous matters. The substance is not yet pure; it contains another peculiar yellow matter that is separated by cold æther, which dissolves the greater part of the mass, and leaves the other portion in the form of a brown powder. By filtering and washing this brown powder with æther, and then evaporating the æthereal solution, both these substances are obtained.

The portion soluble in æther is of a fawn colour: it cannot be sufficiently dried to be pulverized. The other portion is of lighter colour, readily dries, and is easily reduced to a fine powder by trituration. The first M. Couerbe calls *cérancéphalote* and the second *stéaroconote.*

When the alcohol, holding the remaining matters in solution, is filtered through animal charcoal, and is exposed to spontaneous evaporation, it deposits a considerable number of crystals, which are very white and have a greasy lustre: they are to be pressed in fine linen, and the alcohol by evaporation furnishes more crystals, which are to be added to the first.

When the alcohol has been weakened by repeated evaporations, it

becomes turbid, and yields crystals of the same matter mixed with red oil, which precipitates to the bottom of the vessel: it is difficult to obtain this in a pure state. There often comes down with it some solid matters which give it consistence, and which give it the appearance of fat or even of several fatty matters. In order to separate the oil, it must be subjected to a slight pressure in a cloth, and alcohol poured upon it, which leaves the crystals. This alcohol is turbid on account of the oil which it contains. Some æther is to be added to it, which redissolves the oil, and renders the liquor clear, when exposed to spontaneous evaporation. A part of the æther slowly evaporates; the remainder holds the crystalline matter in solution, and allows the oil, as it is formed, to precipitate to the bottom of the liquid. When the stratum is rather thick, it is to be removed by a pipette and filtered, and it is then pure and reddish. This oil M. Couerbe calls *éléancephol*, or oil of the brain.

As to the very abundant portion of the brain remaining after treatment with æther and alcohol, and which the author calls *névriline*, it is partly composed of albumen, coagulated globules, and of a membranous substance soluble in potash.

Analyses of the preceding Substances.

Cérébrote.—M. Vauquelin appears to have been acquainted with this substance, which he has described under the name of *white fatty matter*, and which has since been called *myclocone* by Kühn; but according to some of the characters which M. Vauquelin has assigned to his white fatty matter, it seems that he did not obtain it pure, since he says that it is fusible and viscid, whereas cérébrote is infusible, and does not stain paper. When properly dried at a gentle heat it becomes friable, and may be pulverized; it is soluble in boiling alcohol, and but slightly so when it is cold. The process for extracting it is dependent upon this difference. It does not saponify with a solution of potash or soda, a property also observed by Vauquelin.

Cérébrote is composed of

Carbon	67·818
Hydrogen	11·100
Azote	3·399
Sulphur	2·138
Phosphorus	2·332
Oxygen	13·213
	100·

Vauquelin does not mention the existence of sulphur in it.

Cérancéphalote.—This substance is solid, brown, insoluble in alcohol and in water, but dissolved by 25 times its weight of cold æther. It softens by heat, and without becoming perfectly fluid: when dried it is elastic, like caoutchouc. M. Vauquelin has not mentioned this substance, but Kühn appears to have had a glimpse of it. Sulphuric acid attacks it with great difficulty: nitric acid reduces it to its elements, and converts the sulphur and phosphorus into acids.

It is composed of

Carbon	66·362
Hydrogen	10·034
Azote	3·250
Phosphorus	2·544
Sulphur	1·959
Oxygen	15·851
	100·

Stéaroconote.—This is a fatty matter, which occurs mixed with the preceding. It is of a fawn colour, infusible and insipid, and by combustion gives an acid charcoal. Neither alcohol nor æther dissolves this substance; both the fixed and volatile oils readily dissolve it. Nitric acid takes it up after slight ebullition, and it reappears as a white fat, which is acid, soluble in boiling alcohol, and crystallizes in small laminæ, similar to margaric and stearic acids.

This substance is composed of

Carbon	59·832
Hydrogen	9·246
Azote	9·352
Phosphorus	2·420
Sulphur	2·030
Oxygen	17·110
	99·990

Eléancéphol.—This is a reddish liquid; its taste is disagreeable: it is soluble in æther, fixed and volatile oils, and alcohol, in all proportions. When heated, this substance dissolves the other matters of the brain readily, and these impart consistence to it. Its composition is similar to the preceding.

Cerebral Cholesterine.—A crystallizable fatty matter, which, according to some authors, must be the result of some morbid change. The constant and considerable quantity which M. Couerbe found in the brain induces the belief that it is a widely diffused organic animal element. It is well known that MM. Denis and Boudet have found it in the blood. The cerebral cholesterine is perfectly similar to that of biliary calculi. Their analyses gave M. Couerbe the same results:

Carbon	84·895
Hydrogen	12·099
Oxygen	3·006
	100·

This analysis differs a little from that of M. Chevreul, as follows:

Carbon	85·095
Hydrogen	11·880
Oxygen	3·025
	100·

Journal de Chimie Médicale, Sept. 1834.

VALERIANIC ACID AND ITS SALTS.

It is well known that M. Grote discovered in the distilled water of valerian an acid, which has been examined by M. Peatz (Berzelius, *Traité de Chimie*, tome v. p. 98). M. Tromsdorff has made some new observations respecting this acid.

Valerianic acid is liquid, colourless, limpid, and oleaginous. Its smell strongly resembles that of the root and the essential oil of the *Valeriana officinalis*: the odour is diminished when the acid is combined with a base, but it is never totally lost. The taste of valerianic acid is very strong, very acid, unpleasant, and permanent. If the acid is diluted, it leaves a sweetish after-taste. Its sp. gr. is about 0·944; it remains fluid at 0° Fahr.; it burns, without leaving any residue, with an intense flame; when heated to about 270° it boils; it is soluble in 30 parts of water at 54°; alcohol dissolves it in all proportions, but neither olive oil nor oil of turpentine; it is very soluble in concentrated acetic acid of sp. gr. 1·07; cold sulphuric acid renders it yellow, and decomposes it when hot, sulphurous acid being evolved; nitric acid, even when hot, has not much action upon it.

According to M. Effling, it is composed of

Carbon......	64·96	or 10 atoms	=	764·37
Hydrogen....	9·54	— 18 ——	=	112·31
Oxygen	25·50	— 3 ——	=	300·00
	100·			1176·68

Valerianic acid is prepared by agitating the essential oil of valerian with carbonate of magnesia and water. The mixture is to be afterwards distilled, and by this an oil is obtained which is no longer acid, and the odour of which is less strong than the original oil: a proper quantity of sulphuric acid is to be added to the liquid which remains in the retort, and the distillation is to be repeated. The valerianate is decomposed, and the valerianic acid distils.

Valerianic acid may also be obtained by another process, described in the work of Berzelius. It consists in saturating the distilled water of valerian with carbonate of potash or of soda; distilling to separate the oil; then decomposing with sulphuric acid, to obtain the valerianic acid by distillation.

The oil of valerian may also be treated with potash or soda; then separating the oil, and afterwards the acid.

The valerianates have a peculiar odour; a sweet taste, followed by a sharp one. Some of these salts are unalterable when exposed to the air, some efflorescent, and others deliquescent; they crystallize with different degrees of facility; they are greasy to the touch, and of different degrees of solubility in water; by heat they are decomposed, but there is first a disengagement of a small quantity of acid, which is volatilized without alteration. The stronger acids separate the valerianic acid from its combinations, and this acid decomposes the benzoates and the carbonates.

The valerianates of potash and soda are deliquescent; the valerianate of zinc crystallizes in laminæ during the cooling of a hot solu-

tion, but by spontaneous evaporation acicular crystals are obtained. Valerianate of barytes is an amorphous mass, unalterable in the air; the valerianates of lime and magnesia crystallize in needles, which are not deliquescent. Valerianate of lead deposits in laminar crystals from a hot solution: by evaporating the liquor a thick syrupy fluid is obtained.

With the oxides of mercury valerianic acid forms two salts. The protovalerianate is but little soluble; a saturated boiling solution deposits small needles on cooling. The pervalerianate of mercury is much more soluble: if the solution is boiled with an excess of protoxide, a bright yellow pulverulent subsalt is deposited on standing.—*Journal de Chimie Médicale, Sept.* 1834, p. 473.

HYDROCYANIC ÆTHER.

M. Pelouze obtained this compound by the action of heat upon a mixture of sulphovinate of barytes and cyanuret of potassium. This æther is colourless; has a strong alliaceous smell, is extremely deleterious to the animal œconomy; inflammable; boils at 176° Fahr.; its density is 0·787; is very slightly soluble in water, but combines with alcohol and sulphuric æther in all proportions. It does not precipitate nitrate of silver; in which respect it resembles muriatic æther, which does not decompose this salt until it has been previously destroyed by the action of heat.

Hydrocyanic æther is formed of equal volumes of olefiant gas and cyanogen, condensed to one half: this is indicated by direct analysis and the density of its vapour.—*Ibid.*, p. 486.

DISTILLATION OF TARTARIC AND PYROTARTARIC ACIDS.

M. Pelouze finds that tartaric acid, like other vegetable acids, yields very different products, and in very variable quantity, according to the temperature employed in its distillation.

With a naked fire there are obtained empyreumatic oil, olefiant gas, water, carbonic acid, acetic acid almost crystallizable on account of its great concentration, and a quantity of pyrotartaric acid so small and so mixed with other products that it is difficult to separate it. From about 400° to 570° Fahr. the same products are obtained, but in very different proportions, and the pyrotartaric acid is much more abundant: between 350° and 375° the proportions of pyrotartaric acid and acetic acid increase still more than the traces of empyreumatic oil; but there are sensible quantities of acetic [carbonic ?] acid, carburetted hydrogen, and carbon. By evaporating the product of this distillation crystals are obtained, which it is possible to purify, but by a long and delicate operation. The following is the best process:

Put the compound liquid in which the pyrotartaric acid is dissolved into a glass retort, and distil until the residue has acquired a syrupy consistence; then change the receiver, and continue the distillation to dryness; expose the liquor last distilled to a very low temperature, or to spontaneous evaporation *in vacuo*. In both cases irregular yel-

lowish crystals are obtained, of an empyreumatic odour; these are to be pressed between several folds of filtering paper; they are then to be redissolved in water, and the boiling solution treated with animal charcoal. By these means pure crystals of pyrotartaric acid are obtained on cooling.

Pyrotartaric acid obtained by this process has the following properties: It is white, inodorous, very soluble in alcohol, strongly sour to the taste, like tartaric acid itself. It is fusible at about 212° Fahr., it boils at 360°; and as it decomposes at a temperature a little higher than this, it is difficult to volatilize it without leaving a residue.

A concentrated solution of this acid does not render lime, barytes, or strontia water turbid; it forms in a solution of acetate of lead a very abundant white precipitate, insoluble in water, but very soluble in an excess of acetate; it does not precipitate either neutral acetate or nitrate of lead. None of the following salts are precipitated by free pyrotartaric acid: proto- and per-salts of mercury, persulphate of iron, the salts of lime and barytes, the sulphates of zinc, manganese, and copper. The neutral pyrotartrate of potash is a deliquescent salt. Pyrotartaric acid is represented by C^5, H^8, O^4; by combining with bases it loses an atom of water, and becomes C^5, H^6, O^3.—*Journal de Chimie Médicale, Sept.* 1834, p. 497.

ON THE EXISTENCE OF TITANIUM IN ORGANIC MATTER. BY MR. G. O. REES.

To the Editors of the Philosophical Magazine and Journal of Science.

Gentlemen,

Being lately engaged in the chemical examination of the organs of the human body, I was struck by a peculiar yellow colour which the salts of the renal capsules afforded when subjected to a red heat, this colour gradually disappearing as the mass (which was a fused one) cooled. In order to investigate the cause of this phænomenon, the following experiments were made:

1st. The mass was digested and boiled in water, and the aqueous solution being decanted, was tested with hydrosulphuret of ammonia, which, after a few minutes had elapsed, afforded a scanty dark green precipitate. I now began to suspect the presence of titanium.

2nd. That portion of salts which was insoluble in water was digested in dilute hydrochloric acid; and the solution being neutralized by ammonia, and tested with the hydrosulphuret, afforded a copious dark green precipitate. A second portion of this acid solution was tested with infusion of galls, which produced a reddish brown precipitate, care having been taken to neutralize the excess of acid.

3rd. The matter which was insoluble in water and acid was tested on platina and charcoal before the blowpipe, and afforded a yellow transparent bead in the outer flame, which became of a dark purple colour when heated in the inner flame.

4th. The green sulphuret, on being exposed to heat, afforded a white powder; and a similar effect resulted on exposing it for seven or eight days in the liquor from which it was precipitated.

Some specimens of renal capsule contain but a small proportion of alkaline salts, and will not so easily produce the yellow colour which first attracted my attention, unless an alkaline matter be supplied to them. I have examined several specimens of the salts contained in these glands, and have always procured similar precipitates and reactions, which, when compared side by side with those produced by titanic acid, show a most unequivocal and perfect resemblance.

It would appear that these salts contain an alkaline titanate, besides a portion of free titanic acid, there not being sufficiency of alkali to act on all the acid present.

In two or three specimens which I have examined the salts have appeared quite black and carbonaceous in appearance; but fusion with phosphate of soda or carbonated alkali has always rendered the yellow colour visible. These black ashes exert an alkaline reaction on reddened litmus paper.

It may be noticed that the precipitate procured with infusion of galls is influenced in its colour, not only by the quantity of acid present, but very materially by the degree of concentration of the infusion and solution.

From other experiments lately made, I have reason to believe that titanium exists in other structures besides the renal capsules.

G. O. Rees.

Guy's Hospital, Oct. 3, 1834.

CRYSTALLIZATION OF KALIUM OR POTASSIUM.

Upon unscrewing the lid of a crucible from which a portion of kalium had been distilled, Professor Pleischl found a fragment of kalium lying upon the remaining carbonaceous mass, curved in such a manner as to lead to the conclusion that it had fallen out of the gun-barrel used in the operation. The concave side of this fragment when examined with a powerful lens under a layer of naphtha, exhibited small projecting crystals, the faces of which were all at right angles to each other, strongly resembling those of artificially crystallized bismuth. Professor Pleischl convinced himself that the crystals were kalium by throwing some of them into water, upon which they immediately took fire, burning with a violet flame.—*Baumgärtner's Zeitschrift*, band iii. S. 1.

SCIENTIFIC BOOKS.

Just published,

A Guide to Geology. By Prof. Phillips.

The Calendar for 1834–35 of the Meetings of the Scientific Bodies of London, showing the Time and Place of the Meetings of the principal Learned Societies,—with their Anniversaries, and the Hours at which their Libraries and Museums are open. To be had at the Office of the Lond. and Edinb. Phil. Mag. and Journal.

Meteorological Observations made by Mr. THOMPSON *at the Garden of the Horticultural Society at Chiswick, near London, and by Mr.* VEALL, *at Boston.*

Days of Month. 1834.	Barometer. London. Max.	Barometer. London. Min.	Barometer. Boston. 8½ A.M.	Thermometer. London. Max.	Thermometer. London. Min.	Thermometer. Bost. 8½ A.M.	Wind. Lond.	Wind. Bost.	Rain. Lond.	Rain. Bost.
Sept. 1	29·951	29·918	29·32	67	53	62	SE.	NW.	0·14	...
2	30·123	29·999	29·40	70	50	59	W.	NW.	...	0·02
● 3	30·181	30·047	29·55	73	57	58	S.	W.	...	...
4	30·072	29·847	29·42	77	58	66	S.	W.	...	...
5	29·950	29·895	29·23	72	44	63	SW.	SW.	...	·05
6	30·161	30·031	29·30	69	46	62	W.	W.	...	...
7	30·188	29·590	29·62	70	47	58	SW.	calm	...	·08
8	29·674	29·534	29·16	68	46	61·5	SW.	calm	·36	·04
9	29·663	29·387	28·85	68	49	56	W.	calm	·03	·31
☽ 10	29·807	29·785	29·20	67	55	57	S.	calm	·08	...
11	29·910	29·679	29·30	67	46	57	S.	calm	·05	·13
12	30·326	30·088	29·55	66	46	57·5	W.	NW.	...	·23
13	30·490	30·403	30·01	68	44	54	E.	NW.	...	...
14	30·527	30·422	29·94	68	38	56	E.	calm	...	...
15	30·378	30·151	29·80	74	43	58	SE.	calm	...	...
16	30·038	30·010	29·54	78	54	60	S.	E.	...	...
○ 17	30·109	29·997	29·43	78	54	62	SW.	calm	...	...
18	30·216	30·178	29·58	75	51	62	SW.	calm	·03	...
19	30·267	30·171	29·61	78	53	66	W.	calm	...	...
20	30·345	30.321	29·75	76	56	63·5	NE.	calm	...	...
21	30·348	30.298	29·75	68	53	61	E.	calm	...	...
22	30·293	30·192	29·69	68	41	59	E.	NE.	...	...
23	30·220	30·201	29·73	69	42	51·5	E.	N.	...	...
24	30·201	30·176	29·69	68	39	57	S.	calm	...	...
25	30·204	30·164	29·68	72	51	56·5	S.	calm	...	...
☾ 26	30·087	29·952	29·55	60	54	56	S.	N.	·14	...
27	29·979	29·859	29·27	68	47	58	W.	calm	...	·44
28	30·224	30·088	29·50	70	47	59	SW.	calm	...	...
29	30·273	30·224	29·75	70	40	56	SE.	calm	...	...
30	30·178	30·133	29·70	65	37	57	E.	calm	...	...
	30·527	29·387	29·5[illegible]	78	37	58·9			0·83	1·30

Remarks.

London. — Sept. 1. Cloudy: rain. 2. Fine. 3. Hazy. 4. Very fine. 5. Overcast: fine. 6. Fine. 7. Hazy. 8. Heavy rain. 9. Overcast: fine. 10. Overcast: rain. 11. Hazy: rain. 12. Heavy dew: fine. 13—25. Very fine: the mornings generally foggy. 26. Slight rain. 27. Rain: fine. 28. Hazy: fine. 29, 30. Foggy: very fine.—A finer month of September is, perhaps, not in remembrance.

Boston.—Sept. 1. Fine: rain P.M. 2. Fine. 3. Cloudy. 4. Cloudy; thermometer at 4 P.M. 75°: rain with thunder and lightning P.M. 5. Cloudy. 6. Fine: rain P.M. 7. Cloudy: rain P.M. 8. Cloudy. 9. Cloudy: rain early A.M. 10. Cloudy: rain P.M. 11. Fine: rain P.M. 12. Cloudy. 13—16. Fine. 17. Fine: thermometer 3 P.M. 77°. 18. Foggy. 19, 20. Fine. 21—25. Cloudy. 26. Cloudy: rain P.M. 27. Rain. 28. Fine. 29, 30. Cloudy.

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[THIRD SERIES.]

DECEMBER 1834.

LVII. *On Phosphuretted Hydrogen.* *By* THOMAS GRAHAM, *F.R.S. Edin., Andersonian Professor of Chemistry, and Vice-President of the Philosophical Society of Glasgow.**

FEW substances have been made the subject of experimental inquiry more frequently than the compounds of phosphorus and hydrogen, and none are so remarkable for the various and conflicting results which they have presented to chemists of the greatest acuteness and practical skill. The obscurity which long hung over the subject has been dispelled, however, in a great measure, by the recent investigations of Henry Rose, of Berlin †. Although baffled in his early inquiries, that philosopher returned again and again to the subject, and at last succeeded in determining the chemical functions and true constitution of phosphuretted hydrogen. He has shown it to be analogous to ammonia in chemical character and composition. But hitherto two compounds of phosphorus and hydrogen had generally been admitted to exist, which were believed to differ in composition as they do in properties, one being spontaneously inflammable in atmospheric air, and the other not so. Rose establishes beyond all doubt that these gases are essentially of the same composition, and of the same specific gravity; and, indeed, that they are mutually convertible, each into the other, without any

* Communicated by the Author.

† An account of Rose's experiments on this subject, with a notice of Dr. Dalton's anticipation of his views, will be found in Lond. and Edinb. Phil. Mag. vol. iii. p. 308.—EDIT.

addition or subtraction of matter that could be perceived. In explanation of their possession of different properties, under the same composition, allusion is made by Rose to *isomerism*, or the doctrine that two bodies may exist identical in composition, but differing in properties. Certainly the existence of *two gases* constituted alike, and yet possessing different properties, if established, would afford a firm basis for this doctrine.

It was the importance of the theoretical results which might be looked for, that induced me to attempt to continue the investigation beyond the point to which it has been carried by Rose. Holding the general doctrine of isomerism as problematical, my inquiries were directed to the discovery, in one or other of the gases, of some adventitious matter, to the presence of which the peculiarities of the species might be attributed.

It is to be understood that the spontaneously inflammable gas made use of in my experiments was prepared by the well-known process of heating phosphorus, lime, and water together. This gas is spoken of as "the self-accendible gas," or as "the gas from phosphuret of lime." The other gas, which is not spontaneously inflammable, was prepared by heating hydrated phosphorous acid, or by allowing the preceding species, contained in low receivers, to stand over water for twenty-four hours. It is described as "the non-accendible gas," "the gas from phosphorous acid." The accendibility of the gas was judged of by allowing it to escape in bubbles into the air from the receiver containing it, over either water or mercury. The experiments were all made when the temperature of the atmosphere was between 60° and 70° Fahr.

1. In the process by which the self-accendible gas is procured, free phosphorus distils over, of which a trace in the state of vapour may well be supposed to remain in the gas for some time. Hence the idea has generally presented itself that the free and highly accendible phosphorus present may be the cause of the spontaneous inflammability of the gas. Dr. Dalton, who all along maintained the opinion, which has finally been established by Rose, that the two gases are of the same composition, was in the habit of referring the spontaneous inflammability of the one species to this cause. The speedy loss of the property in question in the case of gas confined over water seemed to favour this view. I find, however, that if a small quantity of phosphuretted hydrogen, when not self-accendible, be added to a confined portion of air, sticks of phosphorus introduced into that air do not smoke,—or that phosphorus has no disposition to combine with oxygen when phosphuretted hydrogen is present. In a transparent mixture

of one volume phosphuretted hydrogen, and one thousand volumes, or any smaller proportion whatever, of air, sticks of phosphorus remain unaffected, but the phosphuretted hydrogen itself always undergoes a slow oxidation. In a mixture of one volume phosphuretted hydrogen and two thousand air, phosphorus smoked strongly for some time, but at a certain period the action ceased, and long before the oxygen of the air was exhausted. A minute proportion of phosphuretted hydrogen is, therefore, sufficient to protect phosphorus from oxidation, in which respect this gas resembles the hydrocarburets and essential oils, which have been shown to be equally efficacious in protecting phosphorus from oxidation. All these bodies appear to act in this respect in one way, namely, by taking the precedence of phosphorus in the process of oxygenation. Phosphorus being, therefore, less oxidable than phosphuretted hydrogen itself, cannot be supposed to take fire, and to inflame the gas, or be the cause of its accendibility at low temperatures.

On sending electric sparks through non-inflammable phosphuretted hydrogen itself, phosphorus is deposited; but the gas, while still cloudy from the phosphorus suspended in it, proved to be non-inflammable on passing it into air.

The loss of accendibility in the case of gas confined over water is certainly wholly unconnected with the deposition of any free phosphorus from the gas which may occur, but is due to the rise of oxygen from the water into the gas. It was observed that water which had been boiled to deprive it of all air, and which was then passed up to self-accendible gas confined over mercury, did not affect the gas in the course of forty-eight hours. In this case, moreover, the gas was agitated with the water. The gas continues in general spontaneously inflammable over mercury for forty-eight hours, and sometimes for three or four days, but ceases to be so in a very short time after the admission of a small proportion of air, particularly if the air be added in a gradual manner. Thus, if to the gas be passed up one twentieth part of its bulk of cork or dry stucco, containing air in its pores, a white smoke appears in the gas, and it ceases to be spontaneously inflammable in the course of a few minutes. The same mass of stucco, warmed before being passed up into the gas, did not produce the same effect. The self-accendible gas always deposits on standing a solid matter of a lively yellow colour, containing phosphorus, but in quantity too minute for analysis. This matter is not acted on by any of the ordinary solvents, such as alcohol, æther, alkalies, muriatic acid; but is destroyed by chlorine-water and by nitric acid. The precipitation of this matter is most rapid in the case of gas over water, and is indicative of deterioration of the gas.

2. The self-accendible gas procured from phosphorus, water, and lime, is always mixed with free hydrogen, varying in quantity from 25 to 50 per cent., while the non-accendible gas from phosphorous acid contains no hydrogen gas, but is pure. Rose concludes, that the spontaneous inflammability of the first species cannot depend upon this hydrogen, for the other species is not made self-accendible by the addition to it of any proportion of free hydrogen. On trying the experiment, however, I obtained a different result. A quantity of gas had lost its self-accendibility by standing over water for two or three hours. To my surprise the addition to this gas of hydrogen, in any proportion from one third of a volume to three volumes, restored the self-accendibility of the gas. Spontaneous inflammability was communicated likewise to the gas procured from phosphorous acid, in some cases, merely by adding hydrogen to it. It was perceived, however, early in the course of the investigation, that hydrogen did not uniformly communicate the property in question, and that its influence depended on something accidental, and not essential to the gas. For instance, the hydrogen which comes over almost pure, towards the end of the process for phosphuretted hydrogen, itself had none of this property; nor did it appear in hydrogen obtained from the following sources: From the electric decomposition of water; from the decomposition of steam by iron; from the action of water on amalgam of potassium; or from the action of the following acids on zinc, namely, muriatic, arsenic, and phosphoric. Even in the case of the action of sulphuric acid on zinc or iron, which had first afforded hydrogen possessing the property in question, it turned out that only the hydrogen evolved at an early period of the action is efficient, while the gas evolved after the vivacity of the action is impaired, is nearly, and sometimes entirely, destitute of any influence. The activity of the hydrogen was, in short, traced to a slight impregnation of nitrous acid vapour which it possessed. The sulphuric acid of commerce always contains a small portion of some acid of nitrogen, probably the hyponitrous, from which I find it cannot be freed by boiling or concentration, continued for any length of time. On quickly mixing sulphuric acid with two or three volumes of water, the presence of nitrous acid is attested by its peculiar odour, and almost certainly by the appearance of brown fumes.

That the hydrogen did not owe the property in question to a trace of nitric oxide, which, combining with oxygen, might by a slight consequent evolution of heat have an effect in kindling the phosphuretted hydrogen, was proved by the fact that the property in question could not be imparted to hydrogen by any proportion of nitric oxide: but to this point there

will be occasion to recur. At an earlier stage in the inquiry, some experiments were made upon the effect of other gases than hydrogen upon phosphuretted hydrogen. None, with the exception of sulphuretted hydrogen, (evolved by the action of sulphuric acid on sulphuret of iron, and which, therefore, contains free hydrogen,) appeared to favour the accendibility of the gas. On the contrary, the addition of all others, and even of hydrogen and sulphuretted hydrogen themselves above a certain proportion, distinctly impeded or destroyed the accendibility of this gas. Thus, one volume phosphuretted hydrogen ceased to be inflammable when mixed with the following proportions of different gases:

With	5	volumes	hydrogen.
	2	———	carbonic acid.
	3	———	nitrogen.
	1	volume	olefiant gas.
	$\frac{1}{2}$	———	sulphuretted hydrogen.
	$\frac{1}{10}$	———	nitric oxide.
	$\frac{1}{20}$	———	muriatic acid.
	$\frac{1}{3}$	———	ammoniacal gas.

It is to be remarked, however, in reference to the preceding table, that some specimens of phosphuretted hydrogen appear to be more highly accendible than others, and that there is considerable latitude in the proportion of foreign gas which may be requisite for destroying the spontaneous inflammability of a given specimen. Often a much smaller portion suffices than is stated in the table. I have found half a volume of carbonic acid or nitrogen to have the effect in certain cases. Of course the introduction of any trace of air with the gases must be carefully guarded against. Nitrous acid, when present in hydrogen in too small a proportion to enable that gas to communicate spontaneous inflammability to phosphuretted hydrogen, or to be perceived by the smell, may be detected by the effect of the hydrogen upon a prepared mixture of non-accendible phosphuretted hydrogen and air, which mixture may be had transparent and quite free from white smoke. The addition of hydrogen to this mixture occasions the immediate appearance of a dense white smoke, the oxidation of the phosphorus being partially induced if an infinitesimal proportion even of nitrous acid exist in the hydrogen. Although the oxidation of the phosphorus takes place at the expense of the air present, and only when air is present, yet the nitrous acid appears to be speedily consumed; the fumes soon ceasing, but appearing again on every subsequent addition of active hydrogen, till several volumes have been added, or till the oxygen of the air present is exhausted.

That the influence of the hydrogen was referrible to the ni-

trous impregnation appeared also from the fact, that phosphuretted hydrogen, which had lost its spontaneous inflammability, was rendered as actively inflammable as ever by passing it, bubble by bubble, into an inverted receiver filled with sulphuric acid recently diluted with three measures of water and cooled. The gas was now capable of igniting spontaneously when passed into air, without the intervention of hydrogen. The same diluted acid lost the smell of nitrous acid by exposure to air in a shallow vessel for a few hours, and thereafter was found unfit for the purpose in question. Phosphuretted hydrogen which had acquired spontaneous inflammability from a nitrous impregnation, appeared to retain that property as long as the phosphuretted hydrogen which is spontaneously inflammable as first prepared. Hydrogen gas which had received a nitrous impregnation by being passed through a diluted sulphuric acid, retained in one case, after being confined for twenty-four hours over water, the power of rendering phosphuretted hydrogen spontaneously inflammable.

From the preceding results and other considerations, it seemed not unlikely that the spontaneous inflammability of phosphuretted hydrogen may be an accidental property, and may depend upon the occasional presence of some foreign body in minute quantity. The inquiry suggests itself, Is there a *peculiar principle* in the self-accendible gas? and if so, what is it?

3. It very soon appeared that a peculiar principle is withdrawn from the gas by *porous absorbents*, such as wood-charcoal and baked clay, which substances are capable of destroying the inflammability of several hundred times their volume of gas. Thus, in one experiment, to five hundred measures of highly accendible phosphuretted hydrogen, one measure of charcoal recently heated to redness, and cooled under the surface of mercury, was passed up. In the course of five minutes, a contraction of eight or ten measures occurred, without any oxidation of the gas, for no air was introduced with the charcoal. The gas was still spontaneously inflammable, but ceased to be so in the course of half an hour. It was found, in fact, by different experiments, that wood-charcoal can absorb about ten times its volume of phosphuretted hydrogen itself; that the phosphuretted hydrogen and the peculiar principle are absorbed indiscriminately at first by the charcoal, but that by and by the peculiar principle comes to be entirely absorbed by the charcoal without any further absorption of phosphuretted hydrogen. When the phosphuretted hydrogen did not exceed fifty or sixty times the bulk of the charcoal, the peculiar principle was entirely withdrawn

in five minutes, so that the gas ceased to be self-accendible. Charcoal which had been drenched in water was without effect upon the gas. On heating the charcoal saturated with gas in a retort filled with water, phosphuretted hydrogen was given off, which, however, was not self-accendible; and all my attempts failed to isolate the peculiar principle by separating it from the charcoal. It was quite clear that the peculiar principle formed but a very small proportion of the phosphuretted hydrogen, evidently much less than one per cent. of the bulk of the gas. Spongy platinum introduced into the gas did not exercise any sensible absorbent effect, and no quantity of it seemed sufficient to withdraw the peculiar principle from a small bulk of phosphuretted hydrogen. Stucco, likewise, was without effect upon the gas, at least when access of air was guarded against at the same time. But both of these substances are known to possess a very low absorbent power.

4. Phosphuretted hydrogen transferred to a receiver over mercury, the inside of which has been moistened by a solution of caustic potash, always loses its spontaneous accendibility, although by no means rapidly, several hours being generally required.

5. Certain *acids* appear to have a remarkable power in withdrawing the principle of inflammability from phosphuretted hydrogen. Let phosphuretted hydrogen be transferred into a jar inverted over mercury, of which jar the inner surface has been moistened with concentrated phosphorous acid. A small quantity of the milk-white matter immediately appears in the acid where exposed to the gas, and in two or three minutes the gas has ceased to be spontaneously inflammable in air, without any appreciable diminution of its volume having occurred. This white matter, although very sensible to the eye, exists only in the most minute quantity. It is not crystalline, and perhaps not even solid. The introduction of concentrated phosphoric acid into the gas was attended by similar phænomena, and the gas lost its spontaneous inflammability in the course of half an hour. A strong solution of arsenic acid acts as rapidly in withdrawing the peculiar principle as phosphorous acid does; but the arsenic acid soon begins to react upon the phosphuretted hydrogen itself, a dark copper-coloured incrustation soon forming upon the surface of the gas-receiver, which matter is probably a phosphuret of arsenic. Concentrated sulphuric acid is capable of absorbing phosphuretted hydrogen itself, which the preceding acids are not; but even sulphuric acid appears to absorb the peculiar principle in the first instance, by a more active affinity than

it exerts on the gas itself. Diluted phosphorous, phosphoric and arsenic acids react in the same manner upon phosphuretted hydrogen, but not so rapidly as the concentrated acids do.

6. The following liquids are capable of dissolving the quantity of phosphuretted hydrogen gas placed against their names at 65° Fahrenheit.

Alcohol (sp. gr. 850)	$\frac{1}{2}$	volume.
Sulphuric æther......	2	volumes.
Oil of turpentine.....	$3\frac{1}{4}$	———

The *essential oils* and most of the *hydrocarburets* appear to withdraw or to negative the peculiar principle in spontaneously inflammable phosphuretted hydrogen, in a rapid manner. If a jar be moistened in the slightest degree with oil of turpentine, coal-tar naphtha, or with the liquid distilled from caoutchouc, and then be used as a receiver for containing self-accendible gas over either water or mercury, the gas is found to lose its spontaneous inflammability in a few minutes. White fumes often appear in the gas at the same time; but these, I am satisfied, are due to the evolution of some gaseous oxygen from the liquids, and only occur in the case of the portion of gas which is first brought into contact with the liquid, and do not appear in the case of subsequent additions of gas, although the liquid remains capable of destroying the spontaneous accendibility of many portions of gas successively exposed to it. It is not easy to decide whether these vapours destroy irrecoverably the peculiar substance of spontaneous inflammability, or merely negative the action of that principle by their presence. I am inclined to think, however, that they destroy that principle, for the action is not so rapid as the diffusion of the vapour through the gas, the impregnation appearing to be fully accomplished, and yet the loss of inflammability not occurring sometimes for two or three minutes afterwards; particularly in the case of naphtha; a portion of that pure liquid in which potassium had been preserved being used in the experiment. A small addition of æther vapour also destroys the inflammability of phosphuretted hydrogen, although a distinct period must elapse before the change occurs, such as a quarter of or half an hour. The action of alcohol vapour is much slower, generally requiring two or three hours. Pure olefiant gas, containing no air, added in the proportion of ten or twenty per cent., eventually destroys the spontaneous inflammability, but requires a period of not less than twenty or thirty hours. Olefiant gas has a negative influence of quite a different character, which has already been alluded to, and which is in action the moment the gases are

mixed, but which does not appear unless the proportion of olefiant gas be very considerable. It is probable that æther vapour and the gaseous hydro-carburets have an influence of the same kind. An astonishingly minute quantity of an essential oil suffices to destroy the inflammability of the gas over mercury, if allowed an hour or two to act. Hence it is very difficult to preserve gas in the inflammable state in the mercurial trough, if any portion of the mercury has been soiled by an essential oil.

7. The action of *potassium* on the peculiar principle is equally remarkable. A most minute quantity of this metal or of its amalgam destroys the self-accendibility of the gas in a few minutes, without occasioning any reduction of volume that could be measured. The fact is, that potassium or its amalgam is without effect upon phosphuretted hydrogen itself at the temperature of the air, neither absorbing nor decomposing the gas; but upon the peculiar principle, the action of this metal is rapid and certain. One grain of potassium amalgamated with fifty pounds of mercury rendered that quantity of mercury quite unfit for retaining gas over it in an inflammable condition for more than a few minutes. In such experiments the interference of naphtha vapour was perfectly excluded. Zinc and tin, either by themselves or in the state of amalgam, have no sensible effect upon the self-accendible gas, at least in a period of five or six hours. Protoxide of mercury speedily withdraws the peculiar principle, but afterwards also reacts slowly upon the gas itself. On the other hand, the peroxide of the same metal is in no way injurious to the self-accendible gas. Arsenious acid in powder acts in the same manner as protoxide of mercury. The solution of protosulphate of iron, if previously boiled, to deprive it of air, is without effect upon the gas. The extraordinary action of potassium, and that also, perhaps, of the essential oils, seemed to point to the existence of an oxygenated principle as the cause of the spontaneous inflammability of phosphuretted hydrogen. It is sufficiently evident that the proportion in which this principle exists, to the whole gas, is exceedingly small, too minute to afford any hope of isolating the principle. The nitrous impregnation, too, which was found adequate to render gas spontaneously inflammable, shows to how minute a quantity of matter the spontaneous inflammability of phosphuretted hydrogen may, at times, be owing. It seemed within the bounds of possibility that the gas might owe its spontaneous inflammability in ordinary circumstances, if not to nitrous acid, at least to some other principle allied to that substance. This led to a careful re-

examination of the properties of phosphuretted hydrogen made inflammable by means of nitrous acid; a subject of much interest, as illustrating the effect of a most minute and almost infinitesimal quantity of foreign matter in communicating to a chemical body so striking a property as spontaneous inflammability. Independently of the light which it may throw upon the constitution of ordinary phosphuretted hydrogen in reference to that property.

8. Phosphuretted hydrogen which had lost all trace of spontaneous inflammability by standiug a day or two over water, or the gas from hydrated phosphorous acid, could be impregnated with nitrous acid and made spontaneously inflammable in various ways. It was ascertained that the gas obtained by either process was affected in the same way. Such gas only, entirely destitute of spontaneous inflammability, was employed in the following experiments.

(1.) The nitrous acid of Dulong may be added directly to the gas over mercury; a glass spherule, or the bore of a short piece of thermometer tube, being filled with the liquid, and passed up to the gas. When *nitric acid* is brought into contact in this manner with the gas, a violent action ensues, but with *nitrous acid* the evolution of white fumes is very slight. The nitrous acid is absorbed in part by the mercury, but this absorption is slow, provided the quantity of gas with which the acid vapour is mixed be considerable. If the quantity of gas primarily impregnated with nitrous acid in the manner described be small, or the impregnation of nitrous acid considerable, the gas exhibits no disposition to smoke or to take fire when passed into air. It has not become spontaneously accendible. On diluting the gas with a large proportion of unimpregnated phosphuretted-hydrogen, no reaction is indicated, but the whole becomes spontaneously inflammable in a high degree. In fact, it was discovered that the gas is not accendible when the nitrous acid exceeds a certain proportion, which is by no means considerable.

(2.) Allow a single drop of nitrous acid to fall into a dry glass jar, which may be of small dimensions: fill the jar with mercury, and invert it, without loss of time, in the mercurial trough. A bubble of gas will collect in the upper part of the jar, which bubble is chiefly nitrous acid vapour. One cubic inch or so, of phosphuretted hydrogen, or of hydrogen itself, may then be added to the gas in the jar; and this is our nitrous impregnating mixture: suppose this mixture to contain one twentieth of its bulk of nitrous acid vapour; the addition of it in any proportion to phosphuretted hydrogen is not attended by the slightest production of white fumes; in fact, no

reaction appears to take place. But the addition of a single bubble of this mixture, not exceeding one tenth of an inch in volume, to five or six cubic inches of phosphuretted hydrogen will render the whole highly accendible, so that every bubble passed into the air will take fire.

(3.) In the last arrangement a drop of the strongest nitric acid may be substituted for the nitrous acid, in the preparation of the impregnating mixture. The nitric acid acts on the mercury, and nitric oxide charged with nitrous acid vapour is collected, which may be diluted with hydrogen as above.

The preceding processes uniformly afford a nitrous impregnating mixture which may be depended upon; but when the experiment is attempted over water, there is not the same certainty of the impregnation being successful. I have often, however, made hydrogen highly suitable for the purpose, by passing it through a column of fluid composed of nitric acid recently diluted with water, provided that the acid had been fuming from the presence of nitrous acid; or by passing hydrogen through recently diluted sulphuric acid, as has already been stated.

In regard to the proper proportion of nitrous acid vapour to the phosphuretted hydrogen, I am satisfied that the proportion most efficacious is somewhere between one part nitrous acid to one thousand, and one to ten thousand phosphuretted hydrogen. One nitrous acid to one hundred gas, or less gas, is never accendible, but becomes so on diluting it with enough of phosphuretted hydrogen.

I was anxious to discover how far nitric oxide interferes in the phænomena. The nitrous acid is never free from, but is always accompanied with, a certain proportion of this gas.

9. *Action of Nitric Oxide.*—In a table formerly given (p. 405.), nitric oxide is set down as preventing the accendibility of the good gas from phosphuret of lime, when the proportion of the first is so great as one tenth of the whole mixture. In fact, the best inflammable gas when mixed with nitric oxide in quantity from two volumes to one tenth of a volume, exhibited no symptom of spontaneous inflammability. The nitric oxide forms red fumes when the mixture meets the air, but the phosphuretted hydrogen does not even smoke; so that the oxidation of the nitric oxide has not a kindling effect upon the phosphuretted hydrogen, but the very reverse. A mixture of one volume nitric oxide with twenty volumes of good phosphuretted hydrogen (self-accendible *per se*) is still self-accendible; the bubble, however, does not take fire the instant it bursts in the air, but after rising to a little height, and then

it explodes with a puff, like loose grains of gunpowder, and not with the usual snap, the oxidation of the nitric oxide preceding by a sensible interval the oxidation of the phosphuretted hydrogen. Nitric oxide in a considerably smaller proportion than $\frac{1}{20}$th of a volume exhibits a sensible effect in retarding the combustion of self-accendible gas, but does not altogether prevent it.

In the case of phosphuretted hydrogen which was not self-accendible, small additions of nitric oxide, such as one to one hundred, to five hundred, to one thousand, or to two thousand volumes phosphuretted hydrogen, did not induce self-accendibility when the nitric oxide employed had been previously washed with caustic alkali. The experiment was tried with three different specimens of washed nitric oxide. But nitric oxide which had not been washed with alkali, particularly if it resulted from a turbulent action of the nitric acid on copper, and came over charged with red fumes, and was withal newly collected, was pretty often efficient in making the gas self-accendible. The proper proportion of such nitric oxide for this purpose was found to be one volume to a quantity between those of one thousand and two thousand volumes of phosphuretted hydrogen. A greater or a less proportion of the nitric oxide failed to produce the desired effect. All these experiments with nitric oxide were made over water.

It is well known that a mixture of phosphuretted hydrogen and nitric oxide may be exploded by a bubble of oxygen gas, a method of firing these gases first practised, I believe, by Dr. Thomson. But Dr. Dalton found pure nitric oxide capable of oxygenating phosphuretted hydrogen in a gradual manner when the two gases are left together, nitrous oxide and nitrogen resulting. Possibly, therefore, it is by acting itself upon phosphuretted hydrogen that nitric oxide prevents atmospheric air from acting upon that gas in our experiments. It is conceivable that the oxygenating action of nitric oxide upon phosphuretted hydrogen may be promoted, like that of air upon the same gas, by the presence of nitrous acid, which will explain Dr. Thomson's experiment.

The impregnating nitrous mixture of the foregoing experiments was not destitute of nitric oxide; but what proves that the efficiency of the mixture did not depend upon the last-mentioned ingredient is the circumstance that the mixture lost its virtue by standing over mercury for a week, during which period the acid vapour was absorbed by the mercury, but the nitric oxide remained, as appeared on admitting air to the gaseous mixture. Hence we may conclude, that when nitric oxide acts in producing inflammability in phosphuretted hy-

drogen, it is from the nitrous acid which it occasionally contains. It is certainly not a little curious that nitric oxide is not equivalent to nitrous acid in producing the change in question upon phosphuretted hydrogen, seeing that the nitric oxide passes immediately into nitrous acid upon meeting air. Whether the negative influence of nitric oxide upon really accendible gas is sufficient to account for this anomaly, I am doubtful. It may be thought that nitrous acid and phosphuretted hydrogen when in contact for a short time, react upon each other, with the production of some entirely new and highly accendible body. But this supposition seems not to quadrate with the fact that the impregnating mixture requires to be diluted by so large a proportion of phosphuretted hydrogen before the whole becomes spontaneously inflammable; nor is it supported by any visible signs of reaction between the nitrous acid and phosphuretted hydrogen. Indeed, nitrous acid vapour appears to be compatible with phosphuretted hydrogen to an extent which could not have been anticipated. Again, that nitrous acid, or at least some acid compound of nitrogen, continues to exist in what we may now call *nitrous phosphuretted hydrogen gas*, appears to be corroborated by the properties which this self-accendible gas is found to possess.

10. *Properties of Nitrous Phosphuretted Hydrogen.*—(1.) This gas loses its self-accendibility when kept over mercury, in a period varying from six to twenty-four hours, according to the amount of nitrous impregnation. It is remarkable that this gas continues in general for a longer time inflammable when confined over water than over mercury, which is the reverse of what occurs with the gas from phosphuret of lime.

(2.) The factitious gas is deprived of its spontaneous inflammability by charcoal and porous absorbents, by essential oils and hydro-carburets, and by amalgam of potassium, and quite as rapidly as is its natural prototype.

(3.) Phosphorous acid and concentrated sulphuric acid appear likewise to withdraw the nitrous principle, although phosphoric acid does not. The agency of these acids probably exemplifies the disposition of nitrous acid to combine with other acids. The action of potassium and of essential oils upon nitrous acid requires no explanation. Potassium has, I find, no action on pure nitric oxide in the cold.

(4.) A cubic inch of this gas, passed up into a receiver of which the inside was moistened with caustic alkali, had its accendibility sensibly impaired in fifteen minutes, but not completely destroyed in less than an hour.

In conclusion, the statement of the above properties is abundantly sufficient to prove that a strong analogy subsists between our nitrous phosphuretted hydrogen and the self-accendible gas which has been so long in the hands of chemists. The peculiar principle of the last, therefore, may be an oxygenated body. That principle cannot be nitrous acid, but it may be a compound of phosphorus and oxygen ($\overset{....}{\underline{P}}$) analogous to nitrous acid. In all the reactions by which self-accendible gas is produced, we have the simultaneous formation of compounds of phosphorus and oxygen, such as the hypophosphorous and phosphoric acids. The compound $\overset{....}{\underline{P}}$ is hypothetic, however, and has not been formed directly. Its existence is only surmised from the parallelism which appears to be established between nitrogen and phosphorus, and between their compounds; phosphuretted hydrogen itself corresponding with ammonia, the phosphoric and phosphorous acids, with the nitric and hyponitrous acids. The peroxide of chlorine of Davy and Stadion ($\overset{....}{\underline{C}}$) corresponds with nitrous acid and with our hypothetic oxide of phosphorus, which we may speak of as the peroxide of phosphorus. The peroxide of phosphorus would appear to resemble the peroxide of chlorine in being acted on more slowly by mercury and by alkalies than is the case with nitrous acid. It is to be admitted, however, that I did not succeed in producing an inflammable phosphuretted hydrogen by the agency of peroxide of chlorine,—that there is no *chlorous phosphuretted hydrogen*. The reason is, peroxide of chlorine is incompatible with phosphuretted hydrogen, reacting upon that gas at the instant of mixture.

As to the mode in which nitrous acid vapour, in a proportion so minute, contributes to the accendibility of phosphuretted hydrogen, I have been able to form no distinct idea. The most likely conjecture is, that the nitrous acid, or resulting hyponitrous acid, combines with some product of the oxygenation of phosphuretted hydrogen, and thereby disposes or promotes the occurrence of that change. The oxygenation of pure hydrogen itself under the influence of a clean plate of platinum, is not promoted in a sensible degree by any nitrous impregnation. The sulphurous and muriatic acid gases, and vapour of acetic acid, appeared to contribute nothing to the accendibility of phosphuretted hydrogen.

Summary.—It appears, then, that there are not two isomeric phosphuretted hydrogens, but that the peculiarities of the gas when spontaneously inflammable depend upon adventitious matter:

That some oxide of nitrogen, which in the present state of our knowledge of that class of compounds seems to be the nitrous acid, is capable of rendering phosphuretted hydrogen spontaneously inflammable, when that oxide is present to the extent of *one ten-thousandth part* of the volume of the gas:

That such gas has a general resemblance to phosphuretted hydrogen as obtained in the spontaneously inflammable state by ordinary processes, which last probably owes its ready accendibility to the presence of an equally minute trace of a volatile compound of phosphorus and oxygen, analogous to nitrous acid.

LVIII. *Observations on Magnetic Substances. By Mr.* David Lyon.*

IRON was for a long space of time the only substance which was known to be capable of exhibiting magnetic phænomena. It was afterwards found that two other metals, nickel and cobalt, are endowed with a similar capacity.

Hence it appears that magnetism is not a peculiar essence of one particular substance only; and that whatever be its nature, it is not confined to one substance alone, but is common to different chemical elements. It may, therefore, be properly inquired, whether in any principal qualities, or in the numerical values of qualities, these elements do resemble one another, and are likewise distinguishable from all the other elements. For if such be the case, we may come to know on what magnetism depends, or in other words, to account for its presence in these elements and in no other; although we do not enter upon the question, as to what magnetism really is, or what is its physical origin.

The atomic weights of the three magnetic elements, as stated by Berzelius, are as follows:

Nickel......................	739·51
Cobalt......................	738·
Iron	678·43

The specific gravities, as stated in Pouillet's *Elémens de Physique*, are,

Nickel......................	8·27
Cobalt......................	7·8
Iron	7·207

Thus, both in the atomic weights and in the specific gravities, we observe that the three magnetic substances or elements have values near together: and on inspecting the values of all the chemical elements, we observe further, that there is no

* Communicated by the Author.

other element whose values come very near or fall within those of the three magnetic elements. This is a fact or observation which appears hitherto not to have been brought forward, but which is certainly deserving of notice, both for itself, and for the considerations to which it may lead.

If, for the several chemical elements, we divide the specific gravities by the atomic weights respectively, we shall obtain a series of numbers, which are the relative numbers of atoms of the several elements contained in a given space or volume. In this way, and making use of the values stated above, we have for the three magnetic elements:

	Specific Gravity.	Atomic Weights.	Atoms contained in a given space.
Nickel......	8·27	739·51	1118
Iron	7·207	678·43	1062
Cobalt......	7·8	738·	1057

It will scarcely need to be noted, with reference to this calculation, that we are at liberty to remove the decimal points, provided it be done *uniformly* for all.

On considering the two series, *i. e.* the atomic weights and the ratios of atoms contained in a given space, respectively, for the several chemical elements, we shall be conducted to speculations relative to the atomic bulks; or in other words, to investigate what may be the relative bulks or sizes of the solid nucleus of the atoms of the several elements. But for our present purpose, still attending solely to the three magnetic elements, when we consider the approximation of their numbers in these two series, there appears no ground for hesitation in allowing that their atomic bulks must be but little different from one another.

We come then to the conclusion that the values of the atomic weights, and likewise of the atomic bulks of the three magnetic elements, are near together*. And we shall find further, that no other element approaches them in both these

* The atomic weights stated above are those published by Berzelius in his *Essai sur la Théorie des Proportions Chimiques*, and copied in the fifth edition of Thénard's *Traité de Chimie*. In a subsequent list, published by Berzelius in the *Annales de Chimie et de Physique*, tom. xxxviii. (1828), and copied in Dr. Daubeny's Introduction to the Atomic Theory, some modifications are made in the atomic weights. The metals generally have numbers assigned to them, which are one half of those in the former list. But they have not *all* been *so* reduced; there are exceptions in some few instances. Now according to this later series, it is further remarkable, on forming as above indicated a table of the numbers of atoms contained in a given space, that of all the chemical elements, with the exception of carbon, the three magnetic elements and copper are those which have the *greatest numbers* in that table.

respects. And are not these two qualities the principal (if not the only) primary qualities which belong to atoms, and may be different in value for the different chemical elements?

May it not be hence inferred, that the power or liability to manifest the magnetic phænomena,—being peculiar to, or most observable in, certain elements, and them only,—is dependent upon the values of those two qualities in their atoms? But what the particular nature may be of the connexion binding the magnetic phænomena to such elements as have values of the two qualities at or near to the mean of the values of the three elements, iron, nickel, and cobalt, may be the subject of further research.

It will thus be observed that in inquiries into magnetism we may distinguish two parts, independent of each other: 1st, the nature of the magnetic substance; 2nd, the nature of the magnetic phænomena, or the real source of their production, and the description of the minute and latent modes of the phænomena themselves. What has been adduced above, relates solely to the former of these parts.

With regard to the latter, and chiefly for the sake of its bearing upon the inference or view above proposed, it may be well to state that I have been engaged in an investigation, which appears to me to disclose the origin or fundamental principle of the phænomena of terrestrial magnetism. It may be added that the original idea of this investigation occurred to me, and was pursued some time previously to either of the inquiries relative to magnetism. Not knowing, however, whether my present communication will be permitted to appear in the pages of the Philosophical Magazine, and also on account of the additional space, and the diagrams that would be required for the development of my views on this subject, the exposition of the same must be deferred for another communication.

For the reason before mentioned, I still think it will be of service to give a brief notice thereof, and have accordingly drawn up the subjoined queries for that purpose.

Whether, in considering more rigorously the principles applicable to the rotation of the globe, *i. e.* on examining more minutely into the actual path of translation of the atoms, there may not be found the origin or real source of production of the phænomena of terrestrial magnetism; it being conceived that this path is not represented *in its ultimate form* by the theory which lays it down as being a circle.

And whether the discussion of this matter will not more immediately (and previously to or apart from any discussion of the agency of an æther, or of the consequences of the hetero-

geneity of atoms in the globe,) afford the natural interpretation of the law or formula of the magnetic dip or inclination deduced by Messrs. Biot and Kraft from data of observation, viz. that for any given latitude, the tangent of the inclination is to the tangent of that latitude as 2 : 1.

If it can thus be shown that the fundamental principle of terrestrial magnetism is necessarily connected with the path of an atom, considered in a system homogeneously composed, then with regard to the various chemical elements or species of atoms composing the mass of the earth, the greater part of which do not exhibit the magnetic phænomena, it would remain to be examined in what manner, or by what circumstance, dependent upon the respective values of the two atomic qualities before mentioned, the atomic paths of the greater number are so far modified (from what may be called the *type*, preserved in the magnetic elements,) as not to present the (magnetic) phænomena in the same sensible way as the three elements, iron, nickel, and cobalt.

In connexion with the foregoing investigations would come another: whether, in general, the physical and chemical properties of the several chemical elements or species of atoms may not depend upon the respective values of the atomic weights and atomic bulks. And here it may be remarked with respect to the magnetic elements, and the approximation in their values of the two qualities, that besides the circumstance of their being magnetic, there are also to be observed many marked analogies in their physical and chemical properties; these analogies being, without doubt, greater than what are to be found in the case of any two or more of the other chemical elements.

Liverpool, April 22, 1834.

LIX. *Description of a Thunder-Storm as observed at Woolwich; with some Observations relative to the Cause of the Deflection of Electric Clouds by high Lands; and an Account of the Phænomena exhibited by means of a Kite elevated during the Storm.* *By* W. STURGEON.*

ON Saturday evening, (June 14th,) about 8 o'clock, an electric storm passed partly over this place, exhibiting lightning the most splendid ever beheld. The wind was pretty brisk from S. by W. about the first appearance of the storm, and if the electric clouds had obeyed the force of the wind only, the principal part of them would have come directly over us. This, however, was not the case, for instead of their being

* Communicated by the Author.

carried over Woolwich in the direction of the wind, the most formidable group of them, and consequently the greatest fury of the storm, were deflected out of the wind's track before their arrival at Shooter's Hill, and were carried over the low lands on the other side of the hill, towards the Thames, in a direction nearly from W.S.W. to E.N.E.

The deflection of electrized clouds out of the wind's direction, though, perhaps, not much noticed, is a very common circumstance in the neighbourhood of high lands, especially if those lands are composed of materials which are bad conductors of electricity. For although they do not absolutely refuse the transmission of the electric matter driven to the surface of the hill from the lower strata of air* by the disturbing force of the condensed electric fluid in the clouds, the transfusion into the ground is too tardily performed to prevent accumulation on the surface, which consequently becomes charged in the same state as the clouds that are approaching it. A reaction immediately takes place, and a con-

* The asperifolious plants, and the vegetable clothing of the land generally, especially at this season of the year, receive the electric fluid from the atmosphere in great abundance; and the myriads of vegetable points, sharp edges, &c., presented to the air, offer every facility for its reception on any emergency of pressure emanating from the repulsive force of a highly charged cloud. The surface of the land thus becomes charged at the expense of the air, each gradually resuming its natural electric equilibrium again, as the disturbing force withdraws its influence, by the progress of the cloud in its course. Thus new tracts of country become charged in succession as the cloud approaches them, and an electrical tide sweeps the face of the land by the floating influence above.

It is on this account that insulated kite-strings, exploring rods, &c., frequently become negatively electric at the approach, and during the transit, of clouds of this description. But if the kite or the exploring rod were to reach into the cloud, it is not likely that either of them would ever be found in a negative state. I am speaking of the principal influencing cloud, and not of those straggling thin patches in their vicinity, which frequently became negative by a portion of the electric matter which they before possessed being driven out of them by the predominating electric force of the superior cloud. In the same manner, an insulated metallic rod furnished with fine points or sharp edges at its further extremity, may have its natural electric fluid driven out of it into the air, by the approximation of a positively charged body at the other end.

I have never yet found the atmosphere *negative* with regard to the earth at any other time than when modified by such causes as I have pointed out. I have made upwards of four hundred electric kite experiments, under almost every circumstance of weather, at various times of the day and night, and in every season of the year; I have experimented on Shooter's Hill, and on the low lands on the Woolwich and Welling sides of it, and the experiments in the three different places within an hour of each other; I have done the same on Chatham lines, and in the valley on the Chatham side of them; on Norwood Hill, and in the plain at Addiscombe; also on the top of the Monument in London, and during

sequent repulsion or deflection of the clouds is produced. This electric force now operating in conjunction with the wind, gives the cloud a new direction of motion, and urges it over a tract of country composed of better conductors, which are more susceptible of being transpierced by the electric matter than those from which the cloud was deflected. Hence it is that electric storms are more frequent and more violent over marshy lands, rivers, &c., than over drier and more elevated tracts of country*.

These causes operated in a very beautiful manner in giving direction to the storm on Saturday evening. The principal group of clouds, as before stated, never reached Shooter's Hill, but was carried over the low wet lands, on the other side, to the Thames; and the foremost clouds were taken to the other side of the river, and over a considerable tract of the Essex marshes. At this period another direction was given to the storm, and the new combination of forces urged it in the direction of the river, a route which electric storms visiting this neighbourhood very frequently take.

Its progress down the river was exceedingly slow, owing, as I suppose, to the wind (though much slackened before this time) being more directly opposed to it. I had been floating an electric kite in the Artillery Barrack-field during the transit of that part of the storm which passed over Woolwich. I had got completely wet with the heavy rain which fell during the time; notwithstanding which, the unusual fineness of the lightning which was playing over the river and marshes induced me to pursue it with my eye, when, from its distance, I could no longer explore the theatre of its resplendent exhi-

the present year, on the top of some of the high hills in Westmoreland and in the North Riding of Yorkshire; and in every case I have found the atmosphere positive with regard to the ground. In most of these cases, the stations at the tops of the hills were higher than the place of the kite when the experiments were made at the lower ones.

I have floated three kites at the same time, at very different altitudes, and have uniformly found the highest to be positive to the other two, and the centre kite positive to that which was below it; consequently the lowest one was negative to the two above it; but still it was positive to the ground on which I was standing. I have made more than twenty experiments of this kind, and the results (with the exception of electric tension) were invariably the same; showing most decidedly that the atmosphere in its undisturbed electric state is more abundantly charged than the earth, and, as far as I have been able to explore it, still more abundantly in the upper than in the lower strata.

* Immense tracts of flat country frequently become charged in the same manner and from the same cause as ranges of hills are charged; but the repulsive force from such places is directed vertically, and not so directly opposed to the horizontal direction of the cloud's motion as that proceeding from the side of a high hill.

bition. I walked to the top of Wellington-street, from which place I had an exceedingly fine view of the storm, now too distant to hear even a feeble murmur of those thunders, which, I am persuaded I may safely affirm, were to the inhabitants immediately in their vicinity terrible to an unusual degree.

It was now 9 o'clock, and the lightning was magnificent indeed. Nature appeared as if disposed to gratify the utmost extent of curiosity by an unremitting display of her electrical elemental fire.

For about half an hour the storm appeared to be nearly stationary, hovering over a tract of low land on the Essex and Kent sides of the Thames, perhaps not far from Purfleet. The lightning was unusually refulgent; the flashes in rapid succession, and discharged in every possible direction that can be imagined, and, generally through a longer striking distance than I had ever before noticed. Three or four discharges which occurred a little after 9 o'clock, darted through a horizontal arch of about 50° each; and several of those which were directed vertically and oblique to the horizon, shot through 30° or 40°, the fluid being visible in every part of the circuit.

If this lightning was discharged over Purfleet, or thereabouts, as I have supposed, it would be about eight miles from where I was standing. Now allowing seven miles to be the mean distance of the lightning discharged in a track at right angles to the line of sight, the angle of 50° would give a chord of nearly six miles and a half for the striking distance, or the tract of air through which the lightning travelled visibly at one discharge. The apparently vertical and oblique discharges were much nearer in some part of the circuits than those which shot through the extraordinary horizontal ranges. The rain, I imagine, was falling in torrents, which would greatly facilitate the transmission through long striking distances. Moreover, the inferior density of the air in the regions of the clouds, and the thin aqueous vapours which are floating there, tend very much to facilitate the transmission.

Buildings, trees, and other tall objects are not usually struck by lightning before the falling of rain, the dry dense air offering too great a resistance to be transpierced from the clouds to the ground.

From the time that the clouds arrived within the influence of the Thames, they seemed to travel nearly in its direction; for although the lightning played over some miles of country on both sides of its banks, the river appeared to be the direc-

tion line to the focus of the storm, which, if not earlier disposed of, would probably be transported by it to the Nore or to the Channel; a direction nearly at right angles to that of the wind at this place.

The lightning was very fine from the straggling clouds which passed over our heads, and from others which crossed the Thames much nearer to London. These clouds separated from the splendid group already noticed, and travelled in the direction of the wind towards the N. or N. by E., and would, if not obstructed, discharge their lightnings as a distinct storm over the country about Waltham, Epping, Chipping Ongar, Harlow, &c.

The wind had abated to such a degree before I arrived in the Barrack-field, and the rain fell so heavily during the time I was there, that it was with some difficulty that I got the kite afloat; and when up, its greatest altitude, I imagine, did not exceed fifty yards. The silken cord also, which had been intended for the insulator, soon became so completely wet that it was no insulator at all. Notwithstanding all these impediments being in the way, I was much gratified with the display of the electric matter issuing from the end of the string to a wire, one end of which was laid on the ground, and the other attached to the silk at about four inches distance from the reel of the kite string. An uninterrupted play of the fluid was seen over the four inches of wet silken cord, not in sparks, but in a bundle of quivering purple ramifications, producing a noise similar to that produced by springing a watchman's rattle. Very large sparks, however, were frequently seen between the lower end of the wire (which rested on the grass) and the ground; and several parts of the string towards the kite, where the wire was broken, were occasionally beautifully illuminated. The noise from the string in the air was like to the hissing of an immense flock of geese, with an occasional rattling or scraping sort of noise.

Two non-commissioned officers of the Royal Artillery were standing by me the whole of the time, who, unaware of the consequence, would very gladly have approached close to the string; and it was not until I had convinced them of the danger of touching, or even coming near to it, at a time when the lightning was playing about us in every direction, that I could dissuade them from gratifying their curiosity too far; probably at the expense of their lives. We anxiously and stedfastly watched what was going on at the end of the string, and the display was beautiful beyond description. The reel was occasionally enveloped in a blaze of purple arborized electrical fire, whose numberless branches ramified over the

silken cord, and through the air to the blades of grass, which also became luminous, on their points and edges, over a surface of some yards in circumference. We also saw a complete globe of fire pass over the silken cord between the wire and reel of the kite-string. The soldiers thought it about the size of a musket-ball. It was exceedingly brilliant, and was the only one that we noticed.

I had no electrometer with me, nor any of the apparatus with which I perform chemical and magnetical experiments by atmospheric electricity; hence the whole of the time on this occasion was devoted to mere observation.

The following is a notice, extracted from the Lancaster Gazette of the 5th of April last, of some experiments made with an electrical kite at Kirkby Lonsdale, in Westmoreland :—

On Saturday the 29th of March, I had a very favourable opportunity of demonstrating experimentally to several of my friends at Kirkby Lousdale, who had attended my recent course of lectures at that place, that an abundance of the electric fluid usually attends hail and snow storms. The wind was pretty brisk, cold, and from the W. by N. nearly the whole of the day. There were several hail showers, each of which, with a simultaneous increase of wind, became a complete transient storm.

During three of these hail-storms, I floated one of my silken electric kites, with a wired string of about 300 yards long, and insulated in the usual way by means of a silken cord.

The kite was elevated in each experiment about ten minutes prior to the arrival of the hail-storm; and the electric state of the atmosphere ascertained, which was found to be so exceedingly feeble that not the slightest spark could be observed. As, however, the cloud from which the hail was falling approached the kite, the fluid from the string presented itself in brilliant sparks to the knuckle; and during the transit of the cloud, became so abundantly discharged to a wire presented to the string, that it struck in rapid succession through a stratum of six inches of air; and through three inches of air, it presented a splendid continuous stream of electric fire. As the cloud receded from the kite, by advancing in its aërial course, the electric discharges became less and less brilliant, and continued to diminish in splendour and energy with the recession of the passing storm, ultimately vanishing altogether by the emergence of the kite from the electric influence of the cloud.

These appearances were exhibited in each experiment, but the display of the electric fire was the most magnificent in the

second, which was during the fiercest hail-storm of that day, and happened between two and three in the afternoon. During an early part of this storm the electric fluid made a continuous rattling noise down the kite-string (in consequence of the wire being broken in several places), and darted from the reel, at the inferior extremity, to greater distances than in either of the other experiments. In one instance it struck over a stick about a yard long, to the hand of a young man named Croft, who was presenting it to the kite-string. Although the remote end of the stick was in connexion with the ground by means of a very wet string, and consequently a considerable discharge must, at the same time, have passed down the wet string to the earth, the shock was so violent as to make Mr. Croft reel and nearly fall; and I have some reason to suppose that it has left an impression on his memory which time will not speedily obliterate. The kite-strings, however, broke very soon afterwards, and consequently the experiments, on that occasion, terminated very abruptly, and unfortunately at a time also when the fire was streaming from the string in the greatest abundance, and with a degree of splendour better imagined than described.

During the third time that the kite was afloat, about two hours after the former, several gentlemen present experienced smart electric shocks direct from the kite-string.

Artillery Place, Woolwich, June 16, 1834.

LX. *Experimental Researches in Electricity.—Seventh Series. By* MICHAEL FARADAY, *D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution; Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c.*

[Continued from p. 344, and concluded.]

822. THE doctrine of *definite electro-chemical action* just laid down, and, I believe, established, leads to some new views of the relations and classifications of bodies associated with or subject to this action. Some of these I shall proceed to consider.

823. In the first place, compound bodies may be separated into two great classes, namely, those which are decomposable by the electric current, and those which are not. Of the latter, some are conductors, others non-conductors, of voltaic electricity*. The former do not depend for their decom-

* I mean here by voltaic electricity, merely electricity from a most abundant source, but having very small intensity.

posability, upon the nature of their elements only; for, of the same two elements, bodies may be formed, of which one shall belong to one class and another to the other class; but probably on the proportions also (697.). It is further remarkable, that with very few, if any, exceptions (414*. 691.), these decomposable bodies are exactly those governed by the remarkable law of conduction I have before described (394.); for that law does not extend to the many compound fusible substances that are excluded from this class. I propose to call bodies of this, the decomposable class, *Electrolytes* (664.).

824. Then, again, the substances into which these divide, under the influence of the electric current, form an exceedingly important general class. They are combining bodies; are directly associated with the fundamental parts of the doctrine of chemical affinity; and have each a definite proportion, in which they are always evolved during electrolytic action. I have proposed to call these bodies generally *ions*, or particularly *anions* and *cations*, according as they appear at the *anode* or *cathode* (665.); and the numbers representing the proportions in which they are evolved *electro-chemical equivalents*. Thus hydrogen, oxygen, chlorine, iodine, lead, tin, are *ions*; the three former are *anions*, the two metals are *cations*, and 1, 8, 36, 125, 104, 58, are their *electro-chemical equivalents* nearly.

825. A summary of certain points already ascertained respecting *electrolytes*, *ions*, and *electro-chemical equivalents*, may be given in the following general form of propositions, without, I hope, including any serious error.

826. i. A single *ion*, i.e. one not in combination with another, will have no tendency to pass to either of the electrodes, and will be perfectly indifferent to the passing current, unless it be itself a compound of more elementary *ions*, and so subject to actual decomposition. Upon this fact is founded much of the proof adduced in favour of the new theory of electro-chemical decomposition, which I put forth in a former series of these Researches (518. &c.).

827. ii. If one *ion* be combined in right proportions (697.) with another strongly opposed to it in its ordinary chemical relations, *i.e.* if an *anion* be combined with a *cation*, then both will travel, the one to the *anode*, the other to the *cathode*, of the decomposing body (530. 542. 547.).

828. iii. If, therefore, an *ion* pass towards one of the electrodes, another *ion* must also be passing simultaneously to the

* For references to the places in which the subjects of the several preceding series are noticed in our Magazine, see the number for September last, present vol., pp. 161, 162, 165, 172. Edit.

other electrode, although, from secondary action, it may not make its appearance (743.).

829. iv. A body decomposable directly by the electric current, *i. e.* an *electrolyte*, must consist of two *ions*, and must also render them up during the act of decomposition.

830. v. There is but one *electrolyte* composed of the same two elementary *ions*; at least such appears to be the fact (697.), dependent upon a law, that *only single electro-chemical equivalents of elementary ions can go to the electrodes, and not multiples.*

831. vi. A body not decomposable when alone, as boracic acid, is not directly decomposable by the electric current when in combination (780.). It may act as an *ion*, going wholly to the *anode* or *cathode*, but does not yield up its elements, except occasionally by a secondary action. Perhaps it is superfluous for me to point out that this proposition has *no relation* to such cases as that of water, which, by the presence of other bodies, is rendered a better conductor of electricity, and *therefore* is more freely decomposed.

832. vii. The nature of the substance of which the electrode is formed, provided it be a conductor, causes no difference in the electro-decomposition, either in kind or degree (807. 813.); but it seriously influences, by secondary action (744.), the state in which the *ions* finally appear. Advantage may be taken of this principle in combining and collecting such *ions* as, if evolved in their free state, would be unmanageable *.

833. viii. A substance which, being used as the electrode, can combine altogether with the *ion* evolved against it, is also, I believe, an *ion*, and combines, in such cases, in the quantity represented by its *electro-chemical equivalent.* All the experiments I have made agree with this view; and it seems to me, at present to result as a necessary consequence. Whether, in the secondary actions that take place where the *ion* acts, not upon the matter of the electrode, but on that which is around it in the liquid (744.), the same consequence follows, will require more extended investigation to determine.

834. ix. Compound *ions* are not necessarily composed of electro-chemical equivalents of simple *ions.* For instance, sulphuric acid, boracic acid, phosphoric acid, are *ions*, but not *electrolytes*, i. e. not composed of electro-chemical equivalents of simple *ions.*

* It will often happen that the electrodes used may be of such a nature as, with the fluid in which they are immersed, to produce an electric current, either according with or opposing that of the voltaic arrangement used, and in this way, or by direct chemical action, may sadly disturb the results. Still, in the midst of all these confusing effects, the electric current, which actually passes in any direction through the decomposing body, will produce its own definite electrolytic action.

835. x. Electro-chemical equivalents are always consistent, *i. e.* the same number which represents the equivalent of a substance A when it is separating from a substance B, will also represent A when separating from a third substance C. Thus, 8 is the electro-chemical equivalent of oxygen, whether separating from hydrogen, or tin, or lead; and 103·5 is the electro-chemical equivalent of lead, whether separating from oxygen, or chlorine, or iodine.

836. xi. Electro-chemical equivalents coincide, and are the same, with ordinary chemical equivalents.

837. By means of experiment and the preceding propositions, a knowledge of *ions* and their electro-chemical equivalents may be obtained in various ways.

838. In the first place, they may be determined directly, as has been done with hydrogen, oxygen, lead, and tin, in the numerous experiments already quoted.

839. In the next place, from propositions ii. and iii. may be deduced the knowledge of many other *ions*, and also their equivalents. When chloride of lead was decomposed, platina being used for both electrodes (395.), there could remain no more doubt that chlorine was passing to the *anode*, although it combined with the platina there, than when the positive electrode, being of plumbago (794.), allowed its evolution in the free state; neither could there, in either case, remain any doubt, that for every 103·5 parts of lead evolved at the *cathode*, 36 parts of chlorine were evolved at the *anode*, for the remaining chloride of lead was unchanged. So also when in a metallic solution one volume of oxygen, or a secondary compound containing that proportion, appeared at the *anode*, no doubt could arise that hydrogen, equivalent to two volumes, had been determined to the *cathode*, although, by a secondary action, it had been employed in reducing oxides of lead, copper, or other metals, to the metallic state. In this manner, then, we learn from the experiments already described in these Researches, that chlorine, iodine, bromine, fluorine, calcium, potassium, strontium, magnesium, manganese, &c., are *ions*, and that their *electro-chemical equivalents* are the same as their *ordinary chemical equivalents*.

840. Propositions iv. and v. extend our means of gaining information. For if a body of known chemical composition is found to be decomposable, and the nature of the substance evolved as a primary or even a secondary result (743. 777.) at one of the electrodes, be ascertained, the electro-chemical equivalent of that body may be deduced from the known constant composition of the substance evolved. Thus, when fused protiodide of tin is decomposed by the voltaic current

(804.), the conclusion may be drawn, that both the iodine and tin are *ions*, and that the proportions in which they combine in the fused compound express their electro-chemical equivalents. Again, with respect to the fused iodide of potassium (805.), it is an electrolyte; and the chemical equivalents will also be the electro-chemical equivalents.

841. If proposition viii. sustain extensive experimental investigation, then it will not only help to confirm the results obtained by the use of the other propositions, but will give abundant original information of its own.

842. In many instances, the *secondary results* obtained by the action of the evolving *ion* on the substances present in the surrounding liquid or solution, will give the electro-chemical equivalent. Thus, in the solution of acetate of lead, and, as far as I have gone, in other proto-salts subjected to the reducing action of the nascent hydrogen at the *cathode*, the metal precipitated has been in the same quantity as if it had been a primary product, (provided no free hydrogen escaped there,) and therefore gave as accurately the number representing its electro-chemical equivalent.

843. Upon this principle it is that secondary results may occasionally be used as measurers of the volta-electric current (706. 740.); but there are not many metallic solutions that answer this purpose well; for unless the metal is easily precipitated, hydrogen will be evolved at the *cathode* and vitiate the result. If a soluble peroxide is formed at the *anode*, or if the precipitated metal crystallize across the solution and touch the positive electrode, similar vitiated results are obtained. I expect to find in some vegetable salts, as the acetates of mercury and zinc, solutions favourable for this use.

844. After the first experimental investigations to establish the definite chemical action of electricity, I have not hesitated to apply the more strict results of chemical analysis to correct the numbers obtained as electrolytical results. This, it is evident, may be done in a great number of cases, without using too much liberty towards the due severity of scientific research. The series of numbers representing electro-chemical equivalents must, like those expressing the ordinary equivalents of chemically acting bodies, remain subject to the continual correction of experiment and sound reasoning.

845. I give the following brief Table of *ions* and their electro-chemical equivalents, rather as a specimen of a first attempt than as anything that can supply the want, which must very quickly be felt, of a full and complete tabular account of this class of bodies. Looking forward to such a table as of extreme utility (if well constructed) in developing the

intimate relation of ordinary chemical affinity to electrical actions, and identifying the two, not to the imagination merely, but to the conviction of the senses and a sound judgement, I may be allowed to express a hope, that the endeavour will always be to make it a table of *real*, and not *hypothetical*, electro-chemical equivalents; for we shall else overrun the facts, and lose all sight and consciousness of the knowledge lying directly in our path.

846. The equivalent numbers do not profess to be exact, and are taken almost entirely from the chemical results of other philosophers in whom I could repose more confidence, as to these points, than in myself.

847. TABLE OF IONS.

Anions.

Oxygen	8	Selenic acid	64	Tartaric acid	66
Chlorine	35·5	Nitric acid	54	Citric acid	58
Iodine	126	Chloric acid	75·5	Oxalic acid	36
Bromine	78·3	Phosphoric acid	35·7	Sulphur (?)	16
Fluorine	18·7	Carbonic acid	22	Selenium (?)	
Cyanogen	26	Boracic acid	24	Sulpho-cyanogen	
Sulphuric acid	40	Acetic acid	51		

Cations.

Hydrogen	1	Copper	31·6	Potassa	47·2
Potassium	39·2	Cadmium	55·8	Soda	31·3
Sodium	23·3	Cerium	46	Lithia	18
Lithium	10	Cobalt	29·5	Baryta	76·7
Barium	68·7	Nickel	29·5	Strontia	51·8
Strontium	43·8	Antimony	64·6 ?	Lime	28·5
Calcium	20·5	Bismuth	71	Magnesia	20·7
Magnesium	12·7	Mercury	200	Alumina	(?)
Manganese	27·7	Silver	108	Protoxides generally.	
Zinc	32·5	Platina	98·6 ?	Quinia	171·6
Tin	57·9	Gold	(?)	Cinchona	160
Lead	103·5	——		Morphia	290
Iron	28	Ammonia	17	Vegeto-alkalies generally.	

848. This Table might be further arranged into groups of such substances as either act with, or replace, each other. Thus, for instance, acids and bases act in relation to each other; but they do not act in association with oxygen, hydrogen, or elementary substances. There is indeed little or no doubt that, when the electrical relations of the particles of matter come to be closely examined, this division must be made. The simple substances, with cyanogen, sulpho-cyanogen, and one or two other compound bodies, will probably form the first group; and the acids and bases, with such analogous compounds as may prove to be *ions*, the second group. Whether these will include all *ions*, or whether a

third class of more complicated results will be required, must be decided by future experiments.

849. It is *probable* that all our present elementary bodies are *ions*, but that is not as yet certain. There are some, such as carbon, phosphorus, nitrogen, silicon, boron, alumium, the right of which to the title of *ion* it is desirable to decide as soon as possible. There are also many compound bodies, and amongst them alumina and silica, which it is desirable to class immediately by unexceptionable experiments. It is also *possible*, that all combinable bodies, compound as well as simple, may enter into the class of *ions*; but at present it does not seem to me probable. Still the experimental evidence I have is so small in proportion to what must gradually accumulate around, and bear upon, this point, that I am afraid to give a strong opinion upon it.

850. I think I cannot deceive myself in considering the doctrine of definite electro-chemical action as of the utmost importance. It touches by its facts more directly and closely than any former fact, or set of facts, have done upon the beautiful idea, that ordinary chemical affinity is a mere consequence of the electrical attractions of the particles of different kinds of matter; and it will probably lead us to the means by which we may enlighten that which is at present so obscure, and either fully demonstrate the truth of the idea, or develop that which ought to replace it.

851. A very valuable use of electro-chemical equivalents will be to decide, in cases of doubt, what is the true chemical equivalent, or definite proportional, or atomic number of a body; for I have such conviction that the power which governs electro-decomposition and ordinary chemical attractions is the same; and such confidence in the overruling influence of those natural laws which render the former definite, as to feel no hesitation in believing that the latter must submit to them also. Such being the case, I can have no doubt that, assuming hydrogen as 1, and dismissing small fractions for the simplicity of expression, the equivalent number or atomic weight of oxygen is 8, of chlorine 36, of bromine 78·4, of lead 103·5, of tin 59, &c., notwithstanding that a very high authority doubles several of these numbers.

§ 13. *On the absolute Quantity of Electricity associated with the Particles or Atoms of Matter.*

852. The theory of definite electrolytical or electro-chemical action appears to me to touch immediately upon the *absolute quantity* of electricity or electric power belonging to different bodies. It is impossible, perhaps, to speak on this point

without committing oneself beyond what present facts will sustain; and yet it is equally impossible, and perhaps would be impolitic, not to reason upon the subject. Although we know nothing of what an atom is, yet we cannot resist forming some idea of a small particle, which represents it to the mind; and though we are in equal, if not greater, ignorance of electricity, so as to be unable to say whether it is a particular matter or matters, or mere motion of ordinary matter, or some third kind of power or agent, yet there is an immensity of facts which justify us in believing that the atoms of matter are in some way endowed or associated with electrical powers, to which they owe their most striking qualities, and amongst them their mutual chemical affinity. As soon as we perceive, through the teaching of Dalton, that chemical powers are, however varied the circumstances in which they are exerted, definite for each body, we learn to estimate the relative degree of force which resides in such bodies: and when upon that knowledge comes the fact, that the electricity, which we appear to be capable of loosening from its habitation for a while, and conveying from place to place, *whilst it retains its chemical force*, can be measured out, and, being so measured, is found to be *as definite in its action* as any of *those portions* which, remaining associated with the particles of matter, give them their *chemical relation*; we seem to have found the link which connects the proportion of that we have evolved to the proportion of that belonging to the particles in their natural state.

853. Now it is wonderful to observe how small a quantity of a compound body is decomposed by a certain portion of electricity. Let us, for instance, consider this and a few other points in relation to water. *One grain* of water acidulated to facilitate conduction, will require an electric current to be continued for three minutes and three quarters of time to effect its decomposition, which current must be powerful enough to retain a platina wire $\frac{1}{104}$ of an inch in thickness*, red hot, in the air, during the whole time; and if interrupted anywhere by charcoal points, will produce a very brilliant and constant star of light. If attention be paid to the instantaneous discharge of electricity of tension, as illustrated in

* I have not stated the length of wire used, because I find by experiment, as would be expected in theory, that it is indifferent. The same quantity of electricity which, passed in a given time, can heat an inch of platina wire of a certain diameter red hot, can also heat a hundred, a thousand, or any length of the same wire to the same degree, provided the cooling circumstances are the same for every part in both cases. This I have proved by the volta-electrometer. I found that whether half an inch

the beautiful experiments of Mr. Wheatstone *, and to what I have said elsewhere on the relation of common and voltaic electricity (371. 375.), it will not be too much to say, that this necessary quantity of electricity is equal to a very powerful flash of lightning. Yet we have it under perfect command; can evolve, direct, and employ it at pleasure; and when it has performed its full work of electrolyzation, it has only separated the elements of a single grain of water.

854. On the other hand, the relation between the conduction of the electricity and the decomposition of the water is so close, that one cannot take place without the other. If the water is altered only in that small degree which consists in its having the solid instead of the fluid state, the conduction is stopped, and the decomposition is stopped with it. Whether the conduction be considered as depending upon the decomposition, or not (413. 703.), still the relation of the two functions is equally intimate and inseparable.

855. Considering this close and twofold relation, namely, that without decomposition transmission of electricity does not occur; and, that for a given definite quantity of electricity passed, an equally definite and constant quantity of water or other matter is decomposed; considering also that the agent, which is electricity, is simply employed in overcoming electrical powers in the body subjected to its action; it seems a probable, and almost a natural consequence, that the quantity which passes is the *equivalent* of, and therefore equal to, that of the particles separated; *i.e.* that if the electrical power which holds the elements of a grain of water in combination, or which makes a grain of oxygen and hydrogen in the right proportions unite into water when they are made to combine, could be thrown into the condition of *a current*, it would exactly equal the current required for the separation of that grain of water into its elements again.

856. This view of the subject gives an almost overwhelming idea of the extraordinary quantity or degree of electric power which naturally belongs to the particles of matter; but it is not inconsistent in the slightest degree with the facts which

or eight inches were retained at one constant temperature of dull redness, equal quantities of water were decomposed in equal times in both cases. When the half-inch was used, only the centre portion of wire was ignited. A fine wire may even be used as a rough but ready regulator of a voltaic current; for if it be made part of the circuit, and the larger wires communicating with it be shifted nearer to or further apart, so as to keep the portion of wire in the circuit sensibly at the same temperature, the current passing through it will be nearly uniform.

* Literary Gazette, 1833, March 1 and 8. Philosophical Magazine, 1833, p. 204. *L'Institut*, 1833, p. 261.

can be brought to bear on this point. To illustrate this I must say a few words on the voltaic pile*.

857. Intending hereafter to apply the results given in this and the preceding series of Researches to a close investigation of the source of electricity in the voltaic instrument, I have refrained from forming any decided opinion on the subject; and without at all meaning to dismiss metallic contact, or the contact of dissimilar substances, being conductors, but not metallic, as if they had nothing to do with the origin of the current, I still am fully of opinion with Davy, that it is at least continued by chemical action, and that the supply constituting the current is almost entirely from that source.

858. Those bodies which, being interposed between the metals of the voltaic pile, render it active, *are all of them electrolytes* (476.); and it cannot but press upon the attention of every one engaged in considering this subject, that in those bodies (so essential to the pile) decomposition and the transmission of a current are so intimately connected, that one cannot happen without the other. This I have shown abundantly in water, and numerous other cases (402. 476.). If, then, a voltaic trough have its extremities connected by a decomposing body, as water, we shall have a continuous current through the apparatus; and whilst it remains in this state may look at the part where the acid is acting upon the plates, and that where the current is acting upon the water, as the reciprocals of each other. In both parts we have the two conditions *inseparable in such bodies as these*, namely, the passing of a current, and decomposition; and this is as true of the cells in the battery as of the water cell; for no voltaic battery has as yet been constructed in which the chemical action is only that of combination; decomposition is always included, and is, I believe, an essential chemical part.

859. But the difference in the two parts of the connected battery, that is, the decomposing or experimental cell, and the acting cells, is simply this. In the former we urge the current through, but it, apparently of necessity, is accompanied by decomposition: in the latter we cause decompositions by ordinary chemical actions, (which are, however, themselves electrical,) and, as a consequence, have the electrical current; and as the decomposition dependent upon the cur-

* By the term voltaic pile, I mean such apparatus or arrangement of metals as up to this time have been called so, and which contain water, brine, acids, or other aqueous solutions or decomposable substances (476.), between their plates. Other kinds of electric apparatus may be hereafter invented, and I hope to construct some not belonging to the class of instruments discovered by Volta.

rent is definite in the former case, so is the current associated with the decomposition also definite in the latter (862. &c.).

860. Let us apply this in support of what I have surmised respecting the enormous electric power of each particle or atom of matter (856.). I showed in a former series of these Researches on the relation by measure of common and voltaic electricity, that two wires, one of platina and one of zinc, each one eighteenth of an inch in diameter, placed five sixteenths of an inch apart, and immersed to the depth of five eighths of an inch in acid, consisting of one drop of oil of vitriol and four ounces of distilled water at a temperature of about 60° Fahr., and connected at the other extremities by a copper wire eighteen feet long, and one eighteenth of an inch in thickness, yielded as much electricity in little more than three seconds of time as a Leyden battery charged by thirty turns of a very large and powerful plate electric machine in full action (371.). This quantity, though sufficient if passed at once through the head of a rat or a cat to have killed it, as by a flash of lightning, was evolved by the mutual action of so small a portion of the zinc wire and water in contact with it, that the loss of weight sustained by either would be inappreciable by our most delicate instruments; and as to the water which could be decomposed by that current, it must have been insensible in quantity, for no trace of hydrogen appeared upon the surface of the platina during those three seconds.

861. What an enormous quantity of electricity, therefore, is required for the decomposition of a single grain of water! We have already seen that it must be in quantity sufficient to sustain a platina wire $\frac{1}{104}$ of an inch in thickness, red hot, in contact with the air for three minutes and three quarters (853.), a quantity which is almost infinitely greater than that which could be evolved by the little standard voltaic arrangement to which I have just referred (860. 371.). I have endeavoured to make a comparison by the loss of weight of such a wire in a given time in such an acid, according to a principle and experiment to be almost immediately described (862.); but the proportion is so high, that I am almost afraid to mention it. It would appear that 800,000 such charges of the Leyden battery as I have referred to above, would be necessary to supply electricity sufficient to decompose a single grain of water; or, if I am right, to equal the quantity of electricity which is naturally associated with the elements of that grain of water, endowing them with their mutual chemical affinity.

862. In further proof of this high electric condition of the

particles of matter, and the *identity as to quantity, of that belonging to them with that necessary for their separation*, I will describe an experiment of great simplicity but extreme beauty, when viewed in relation to the evolution of an electric current and its decomposing powers.

863. A dilute sulphuric acid, made by adding about one part by measure of oil of vitriol to thirty parts of water, will act energetically upon a piece of plate zinc in its ordinary and simple state; but, as Mr. Sturgeon has shown*, not at all, or scarcely so, if the surface of the metal has in the first instance been amalgamated; yet the amalgamated zinc will act powerfully with platina as an electromotor, hydrogen being evolved on the surface of the latter metal, as the zinc is oxidized and dissolved. The amalgamation is best effected by sprinkling a few drops of mercury upon the surface of the zinc, the latter being moistened with the dilute acid, and rubbing with the fingers so as to extend the liquid metal over the whole of the surface. Any mercury in excess forming liquid drops upon the zinc, should be wiped off†.

864. Two plates of zinc thus amalgamated were dried and accurately weighed; one, which we will call A, weighed 163·1 grains; the other, to be called B, weighed 148·3 grains. They were about five inches long, and 0·4 of an inch wide. An earthenware pneumatic trough was filled with dilute sulphuric acid, of the strength just described (863.), and a gas jar, also filled with the acid, inverted in it‡. A plate of platina of nearly the same length, but about three times as wide as the zinc plates, was put up into this jar. The zinc plate A was also introduced into the jar, and brought in contact with the platina, and at the same moment the plate B was put into the acid of the trough, but out of contact with other metallic matter.

865. Strong action immediately occurred in the jar upon the contact of the zinc and platina plates. Hydrogen gas rose from the platina, and was collected in the jar, but no hydrogen or other gas rose from *either* zinc plate. In about ten or twelve minutes, sufficient hydrogen having been collected, the experiment was stopped; during its progress a few small

* Recent Experimental Researches, &c., 1830, p. 74, &c.

† The experiment may be made with pure zinc, which, as chemists well know, is but slightly acted upon by dilute sulphuric acid in comparison with ordinary zinc, which during the action is subject to an infinity of voltaic actions. See De la Rive on this subject, *Bibliothèque Universelle*, 1830, p. 391.

‡ The acid was left during a night with a small piece of unamalgamated zinc in it, for the purpose of evolving such air as might be inclined to separate, and bringing the whole into a constant state.

bubbles had appeared upon the plate B, but none upon plate A. The plates were washed in distilled water, dried, and re-weighed. Plate B weighed 148·3 grains, as before, having lost nothing by the direct chemical action of the acid. Plate A weighed 154·65 grains, 8·45 grains of it having been oxidized and dissolved during the experiment.

866. The hydrogen gas was next transferred to a water-trough and measured; it amounted to 12·5 cubic inches, the temperature being 52°, and the barometer 29·2 inches. This quantity, corrected for temperature, pressure, and moisture, becomes 12·15453 cubic inches of dry hydrogen at mean temperature and pressure: which, increased by one half for the oxygen that must have gone to the *anode*, *i. e.* to the zinc, gives 18·232 cubic inches as the quantity of oxygen and hydrogen evolved from the water decomposed by the electric current. According to the estimate of the weight of the mixed gas before adopted (791.), this volume is equal to 2·3535544 grains, which therefore is the weight of water decomposed; and this quantity is to 8·45, the quantity of zinc oxidized, as 9 is to 32·31. Now taking 9 as the equivalent number of water, the number 32·5 is given as the equivalent number of zinc; a coincidence sufficiently near to show, what indeed could not but happen, that for an equivalent of zinc oxidized an equivalent of water must be decomposed*.

867. But let us observe *how* the water is decomposed. It is electrolyzed, *i. e.* is decomposed voltaically, and not in the ordinary manner (as to appearance) of chemical decompositions; for the oxygen appears at the *anode* and the hydrogen at the *cathode* of the decomposing body, and these were in many parts of the experiment above an inch asunder. Again, the ordinary chemical affinity was not enough under the circumstances to effect the decomposition of the water, as was abundantly proved by the inaction on plate B; the voltaic current was essential. And to prevent any idea that the chemical affinity was almost sufficient to decompose the water, and that a smaller current of electricity might, under the circumstances, cause the hydrogen to pass to the *cathode*, I need only refer to the results which I have given (807. 813.) to show that the chemical action at the electrodes has not the slightest influence over the *quantities* of water or other substances decomposed between them, but that they are entirely dependent upon the quantity of electricity which passes.

868. What, then, follows as a necessary consequence of the whole experiment? Why, this: that the chemical action upon

* The experiment was repeated several times with the same results.

32·31 parts, or one equivalent of zinc, in this simple voltaic circle, was able to evolve such quantity of electricity in the form of a current as, passing through water, should decompose 9 parts, or one equivalent of that substance: and, considering the definite relations of electricity as developed in the preceding parts of the present paper, the results prove that the quantity of electricity which, being naturally associated with the particles of matter, gives them their combining power, is able, when thrown into a current, to separate those particles from their state of combination; or, in other words, that *the electricity which decomposes, and that which is evolved by the decomposition of a certain quantity of matter, are alike.*

869. The harmony which this theory of the definite evolution and the equivalent definite action of electricity introduces into the associated theories of definite proportions and electro-chemical affinity, is very great. According to it, the equivalent weights of bodies are simply those quantities of them which contain equal quantities of electricity, or have naturally equal electric powers; it being the ELECTRICITY which *determines* the equivalent number, *because* it determines the combining force. Or, if we adopt the atomic theory or phraseology, then the atoms of bodies which are equivalents to each other in their ordinary chemical action, have equal quantities of electricity naturally associated with them. But I must confess I am jealous of the term *atom*; for though it is very easy to talk of atoms, it is very difficult to form a clear idea of their nature, especially when compound bodies are under consideration.

870. I cannot refrain from recalling here the beautiful idea put forth, I believe, by Berzelius (703.) in his development of his views of the electro-chemical theory of affinity, that the heat and light evolved during cases of powerful combination are the consequence of the electric discharge which is at the moment taking place. The idea is in perfect accordance with the view I have taken of the *quantity* of electricity associated with the particles of matter.

871. In this exposition of the law of the definite action of electricity, and its corresponding definite proportion in the particles of bodies, I do not pretend to have brought, as yet, every case of chemical or electro-chemical action under its dominion. There are numerous considerations of a theoretical nature, especially respecting the compound particles of matter and the resulting electrical forces which they ought to possess, which I hope will gradually receive their development; and there are numerous experimental cases, as, for instance, those of compounds formed by weak affinities, the

simultaneous decomposition of water and salts, &c., which still require investigation. But whatever the results on these and numerous other points may be, I do not believe that the facts which I have advanced, or even the general laws deduced from them, will suffer any serious change; and they are of sufficient importance to justify their publication, even though much may remain imperfect or undone. Indeed, it is the great beauty of our science, CHEMISTRY, that advancement in it, whether in a degree great or small, instead of exhausting the subjects of research, opens the doors to further and more abundant knowledge, overflowing with beauty and utility, to those who will be at the easy personal pains of undertaking its experimental investigation.

872. The definite production of electricity (868.) in association with its definite action proves, I think, that the current of electricity in the voltaic pile is sustained by chemical decomposition, or rather by chemical action, and not by contact only. But here, as elsewhere (857.), I beg to reserve my opinion as to the real action of contact, not having yet been able to make up my mind as to its being either an exciting cause of the current, or merely necessary to allow of the conduction of electricity, otherwise generated, from one metal to the other.

873. But admitting that chemical action is the source of electricity, what an infinitely small fraction of that which is active do we obtain and employ in our voltaic batteries! Zinc and platina wires, one eighteenth of an inch in diameter and about half an inch long, dipped into dilute sulphuric acid, so weak that it is not sensibly sour to the tongue, or scarcely to our most delicate test papers, will evolve more electricity in one twentieth of a minute (860.) than any man would willingly allow to pass through his body at once. The chemical action of a grain of water upon four grains of zinc can evolve electricity equal in quantity to that of a powerful thunder-storm (868. 861.). Nor is it merely true that the quantity is active; it can be directed and made to perform its full equivalent duty (867. &c.). Is there not, then, great reason to hope and believe that, by a closer *experimental* investigation of the principles which govern the development and action of this subtile agent, we shall be able to increase the power of our batteries, or invent new instruments which shall a thousandfold surpass in energy those which we at present possess?

874. Here for a while I must leave the consideration of the *definite chemical action of electricity*. But before I dismiss this series of experimental researches, I would call to mind that, in a former series, I showed that the current of electricity

was also *definite in its magnetic action* (366. 367. 376. 377.); and, though this result was not pursued to any extent, I have no doubt that the success which has attended the development of the chemical effects is not more than would accompany an investigation of the magnetic phænomena.

Royal Institution, Dec. 31, 1833.

LXI. *Experiments and Observations on the Application of Photometry to certain Cases connected with the Undulatory Theory of Light. By* J. H. Wheeler, *Esq.**

THE interest inspired by the discussions which have arisen within the last few years on the relative claims of the corpuscular and undulatory theories of light, has been productive of inestimable benefit to science. To Mr. R. Potter the scientific world is certainly indebted for bringing forward the first formidable objections, grounded on the application of mathematical reasoning, to highly original experimental evidence; and whatever opinion may be entertained of the result of the controversy, and the light thrown upon the question by the various replies and rejoinders which have appeared, the excellence of Mr. Potter's experimental labours, and the profound ability with which he has maintained his side in the controversy, cannot be too highly appreciated. And it eminently deserves to be remembered, that with the most perfectly philosophic love of truth, he has in a more recent paper candidly allowed the result of a long and laborious series of experiments to be in favour of the theory he set out by opposing. (See this Journal, November 1833.) Still, however, some of the principal objections brought forward by this gentleman *remain unanswered.* I allude to those which depend upon the calculated *intensities* of light in certain cases, which are found totally to disagree with the observed intensities as determined by his highly ingenious and admirably conducted experiments, depending on the application of photometry, which have appeared in several Numbers of this Journal.

Having felt from the first a deep interest in the subject, and especially as connected with *photometry,* I have ever since the publication of Mr. Potter's experiments been engaged, as much as my other avocations would allow, in attempts to repeat and verify them, and, if possible, to extend their application to other cases. My labours hitherto have met with but limited success, partly from the want of sufficient experimental skill, but chiefly from some apparently insuperable

* Communicated by the Author.

difficulties in the *calculation* of the results, which I am obliged to confess I cannot bring into accordance with those of Mr. Potter. My object, then, in the present communication will be to state the results I have arrived at, such as they are, in the hope that they will receive a candid examination. And in order to afford all facilities for tracing out the cause of error wherever it may lie concealed, and disentangling the subject from its perplexities, I will first lay down the simple elementary principles by which I have been guided in my calculations, and which appear to me to involve the correct principles of this branch of optical science.

The fundamental principle on which all photometrical comparisons must proceed is easily seen to be, the equalization of the illuminating effects as estimated by the eye, and the comparison of the distances of the luminous bodies necessary to produce that equalization.

Now the *illuminating effect* is due jointly to the *absolute brightness* of the luminous points, and the *number* of them; or, in other words, the product of the *intensity* and the *quantity* of light.

For the *same light* at different distances (d), the intensity (i) owing to the diffusion of the rays is inversely as d^2, but at the same time the apparent area into which the rays are condensed diminishes, or the condensation increases, in the same ratio; hence on both considerations the *intensity* is constant for all distances.

For *different* lights, the *absolute intensity* of each being thus constant for all distances, the *quantity* of light varies with the absolute area (a) directly, and the apparent visual area inversely; thus, for the *illuminating* effect (I), we shall have

$$\mathrm{I} = \frac{i\,a}{d^2}.$$

If we are examining the light reflected from a given surface inclined to the direction of vision, it will be easily seen that the inclination is the complement of the angle of reflection (ϕ), and that instead of the absolute area (a) we must take for the *effective* area $a \,.\, \cos\phi$, or the illuminating effect will be

$$\mathrm{I} = \frac{i \,.\, a \,.\, \cos\phi}{d^2}.$$

And in comparing two lights, we shall have as a general formula, to be modified to suit the particular cases which may arise, (accenting the letters for the second light,)

$$\frac{\mathrm{I}}{\mathrm{I}'} = \frac{i \,.\, a \,.\, \cos\phi \;\, d'^2}{i' \,.\, a' \,.\, \cos\phi' \;\, d^2}.$$

My first object was the comparison of the illumination in the dark and bright parts of Newton's rings as in Mr. Potter's experiments; but here there are several difficulties of no small magnitude to be encountered: first, we must suppose *equal portions* of each dark and bright space, isolated so as to become the fair objects of comparison for the intensity of light they respectively give. In the next place, there is the radical difficulty in using the apparatus which Mr. Potter so candidly allows, which arises from the necessity for transferring the eye from the inclined glasses to the rings alternately. For all these difficulties, indeed, as Mr. Potter has justly observed, there is no remedy but steady and repeated practice. By this means, but certainly by no other, can the eye become sufficiently accustomed to the work to be able to form an unhesitating decision, and to give confident approximations, at least, to correct comparisons. After a tolerably long experience in this sort of experiment, I must candidly own that I have never been able to feel that entire and perfect confidence in such determinations which would suffice for resting upon them any very nice and delicate conclusions. Yet I agree with Mr. Potter, that long practice will enable us to attain a degree of precision which at first sight would have appeared hopeless.

I will not, however, dwell upon these points, but proceed at once to detail my experiments, in which, with every degree of care and caution, I have, owing to some cause at present quite inexplicable to me, arrived at results very different from those of Mr. Potter.

It will not be necessary for me to detail the construction of my apparatus, because it was in every point precisely the same as Mr. Potter's, for which I would refer the reader to his paper in this Journal, September 1832, [Lond. and Edinb. Phil. Mag. vol. i.] p. 174. I will simply proceed to give the results of my experiments.

In this case the absolute areas of the two portions of inclined glasses are the same, or (using the accented letters to apply to the second glass) $a = a'$, and consequently by the foregoing principles, we shall have (since $d = d'$)

$$\frac{I}{I'} = \frac{i \cos \phi}{i' \cos \phi'},$$

In order to find i and i' I have adopted Mr. Potter's valuable and curious formula for glass, which is, in fact, the basis of all his calculations of these experiments; and I thus find the following results, which will be best stated in a tabular form.

Homogeneous Red Light.		
Glass for the Dark Ring. Incidence.	Glass for e Light Ring. Incidence.	Ratio $\frac{I}{I'}$ by Formula above.
30°	70°	1·267
30	71	1·25
40	73	1·33
40	74	1·33

The results in this last column differ so widely from Mr. Potter's, that I was led to examine several times over the whole calculation; but have been able to detect nothing which can have led to error. I can therefore merely give my results as they stand, and those interested in the subject may possibly be able to throw some light upon the point by further examination.

I will only add, that with regard to the undulatory theory, Mr. Potter's calculation of the ratio of illumination from that theory, viz. 1·1538, approaches so much more nearly to agreement with my results than he found it to do with his, that perhaps we shall find the error (as there must be one somewhere) will in time show that there is in reality a still more close accordance between theory and experiment. Indeed, I will add that as Mr. Potter's calculation is founded upon his estimate of $\frac{1}{30}$th of the incident light being reflected from glass, so from some trials of my own, (which I do not doubt are of far inferior accuracy,) I found the proportion more nearly $\frac{1}{20}$th. Now this was quite independent of the last experiments. And it is curious, that on calculation with this number, in the way adopted by Mr. Potter, I find the resulting ratio from theory about 1·25, which accords exactly with these experiments.

I made other sets of experiments, as Mr. Potter did, to compare the illumination from reflection at the surfaces of several different substances, with the view of comparing the results with the expression for such illumination derived from the undulatory theory.

In the first instance I compared the effects of crown glass and of a diamond possessing a tolerably good plane surface, to the area of which that of the glass was carefully equalized: we have then to find i' (for glass), as before, by Mr. Potter's formula, and since we here equalize the lights, or make $I = I'$, we have for diamond $i = i' \frac{\cos \phi'}{\cos \phi}$.

The following are the results:

Incidence on Diamond.	Corresponding Incidence on Glass.	Ratio of Reflection by Formula.
3°	63°	20·6
10	63 48′	18·8
30	67	24·5
50	73 40′	27·9
70	79	49·3

I have not thought it necessary to follow out these results through so long a series of incidences as Mr. Potter has done; but for the purpose of the results here aimed at, I have no doubt these comparisons will be deemed sufficient. Here again my resulting ratios differ most widely from those of Mr. Potter; yet they will be found to give (for the case of perpendicular incidence) an agreement with theory, which is fully as exact as can be expected from experiments of this nature. The ratio from the undulatory theory as calculated by Mr. Potter, being in this case 18·36, by my experiments it is 20·6.

Another set of experiments, conducted and calculated precisely in the same way, gave me very similar results for a surface of glass of antimony.

Incidence on Glass of Antimony.	Corresponding Incidence on Glass.	Ratio of Reflection by Formula.
0°	61°	16·4
10	61	16·6
50	66	17·
80	80	36·

For perpendicular incidence the undulatory theory gives the ratio 13·33.

I have made other experiments with several substances, which I will not now detail, as my principal object is answered if I can draw attention to these results, so as to induce any one sufficiently versed in the subject to investigate the source of error, which somewhere and somehow has certainly crept in, to give rise to the remarkable discrepancy between my results and those of Mr. Potter. Our data of observation, it will be seen, agree in a singularly close manner, and the trifling errors of observation are such as can in no way account

for the differences in our results. It seems only possible to account for it by some oversight in the calculations; but where it lies I am totally unable to say. I have given at large the principles on which my calculations are founded, which, I believe, are in a mathematical point of view correct. At any rate the subject eminently deserves further inquiry, which I hope and trust it may receive from those who are much better qualified than myself to do justice to it. Professional avocations have, indeed, hindered me from going into as accurate an examination of it as I could wish, and will, I fear, (for some time at least,) prevent me from entering upon a similar repetition of those other highly interesting photometrical researches, which relate to the reflection from metallic surfaces, in which Mr. Potter has found such curious anomalies. These, however, are not so immediately connected with the object I had in view, which chiefly refers to the controversy in regard to the undulatory theory. The experiments, however, on reflection from *glass* at different incidences have a stronger claim to attentive examination, as forming the basis of the calculation in all the other instances.

Queen Street, Nov. 1, 1834.

LXII. *Additional Observations respecting the Magneto-electric Spark and Shock. By* MICHAEL FARADAY, *Esq., D.C.L., F.R.S., &c.*

To Richard Phillips, Esq., F.R.S., &c.

My dear Sir,

LIKE most things done in haste, my letter to you last month involves several errors, some from want of attention, others from want of knowledge. Will you do me the favour to print the present in correction of them?

The first error consists in supposing the electricity of the shock and the electricity of the spark (obtained at the moment of disjunction) are due to different currents, page 351. They are, as I find by careful experiments, due to the same current, namely, one produced by an inductive action at the moment when the current from the electromotor ceases.

If at p. 351, line 26, after "set in motion" be inserted "through the body;" and at line 31, for "counter" be read "second", and if the above statement be allowed to stand for that at the top of page 352, this error will be corrected.

The experimental results which I anticipated, page 352, lines 31—35, and page 354, lines 26—32, do not occur except under peculiar circumstances, and I am now aware why, for natural reasons, they should not. All the effects, in fact,

belong to the inductive action of currents of electricity described in the first section of the first series of my Experimental Researches. I have investigated them to a considerable extent, and find they lead to some exceedingly remarkable and novel consequences. I have still some points to verify, and shall then think it my duty to lay them (in continuation of my first paper) before the Royal Society.

I am, my dear Sir, very truly yours,

Royal Institution, Nov. 20, 1834. MICHAEL FARADAY.

LXIII. *On the Action of Oxalic Acid upon Chloride of Sodium. By Mr.* ARTHUR THOROLD WOOD.*

IT is not generally known that the decomposition of chloride of sodium may be readily effected by the action of oxalic acid; indeed, it is presumed that the fact is altogether new, as our best chemical authors make no mention of it.

That such decomposition does take place may be proved by several simple experiments: for example, if chloride of sodium and oxalic acid be distilled with water in a retort, muriatic acid will pass over; and when its production ceases, upon allowing the contents of the retort to cool, a salt having the characters of oxalate of soda will crystallize, mixed to a certain extent with chloride of sodium according to the proportions of the materials employed.

As the muriatic acid in this experiment is very dilute, and as oxalic acid cannot exist in the free state without a certain proportion of water, the fact of the decomposition of chloride of sodium may be more satisfactorily proved by taking oxalic acid in crystals, and fused chloride of sodium, reducing them to fine powder, and heating them in a glass tube containing a slip of litmus paper, when muriatic acid will be instantly and copiously evolved, and the litmus paper, of course, reddened. When the evolution of the muriatic acid gas ceases, the dry contents of the tube may be transferred to a piece of platinum foil, and intensely heated before the blow-pipe flame; the oxalate of soda will decompose, and carbonate of soda will remain in sufficient quantity to restore the blue of the reddened litmus paper when wetted with water, or to change the yellow of turmeric paper to brown.

If it be merely required to show that alkali is evolved from chloride of sodium by the oxalic acid, it may be done simply by heating the two, folded in a bit of platinum, for a minute or so before the blow-pipe flame, then adding a drop of water and applying the usual test papers.

* Communicated by the Author.

These experiments have not as yet been performed with minute attention as to the proportions of the acting bodies, although such investigation is contemplated; but it is concluded, that the chlorine of the chloride of sodium obtains hydrogen from the water of the oxalic acid to evolve muriatic acid gas, and that the sodium, obtaining its oxygen from the oxygen of the water, forms soda, which combines with the oxalic acid, forming oxalate of soda, decomposable at a red heat into carbonate of soda.

The chloride of calcium also undergoes decomposition when heated with oxalic acid, evolving muriatic acid gas, and forming oxalate of lime, which upon the further continuance of the heat leaves lime.

These researches will be extended to the other chlorides, with the view of getting new results; in the mean time it is hoped that the facts now stated possess some claim to originality.

LXIV. *On Subterranean Temperature, as observed at a Depth of Five Hundred Yards below the Level of the Sea, in Latitude 54° 55′ North, November 15, 1834. By* John Phillips, *F.R.S., F.G.S., Professor of Geology in King's College, London.**

1. THOUGH, upon a review of the facts and reasonings concerning the interior temperature of the globe, we may freely admit that below a certain depth from the surface the thermometric heat becomes continually greater as we descend, so many sources of embarrassment occur in the prosecution of experiments, that it is by no means an unreasonable scepticism to doubt, whether the law of the augmentation of heat in proportion to the depth is even approximately known. Those who, from local and practical experience, are the best enabled to judge of the corrections required for the effects of respiration, light, friction, and chemical action, on the one hand, and of the ventilating air on the other, must allow that the interference of these causes of error, though less considerable than is sometimes imagined, is of serious consequence in so delicate an inquiry.

2. Immediately after leaving Edinburgh, in October, I was at Newcastle for a week, and was informed by Mr. Hutton, of

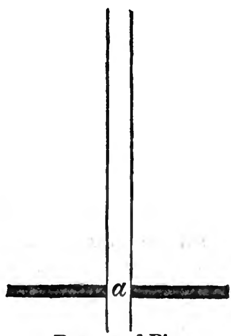
* Communicated by the Author: in the Phil. Mag. first series, for 1823, vol. lxi. p. 347, 436, and vol. lxii. p. 38, 94, will be found a review, drawn up by Mr. Brayley, Jun., of the experiments on Subterranean Temperature which had then recently been made in the Mines of Cornwall and the North of England, exhibited in a tabular form. See also vol. lxvii. p. 302, and Phil. Mag. and Annals, N.S. vol. ix. p. 94.

the spirited attempt of Mr. Pemberton to reach the coal-seams which underlay the magnesian limestone of Durham, by a shaft at Monk-Wearmouth which had then reached the extraordinary depth of 250 fathoms. I mentioned to M. Arago, while he was in Newcastle, my great desire to examine in this pit, the question of subterranean temperature, before the ordinary processes of a deep colliery had complicated the investigation with unascertainable conditions; and was happy to find that a view which I had formed, of relying very much on trials made in subterranean small feeders of water, met with his approval.

3. Professor Whitley, of Durham, had also considered the subject in another point of view, and on my return to Newcastle, I found my friend Mr. W. L. Wharton, of Dryburn, engaged in arrangements for a course of experiments in the pit, which, at the enormous depth of 264 fathoms, had passed through a seam of coal 6 feet in thickness. Every facility being kindly offered by the proprietors of the colliery, (Messrs. Pemberton and Mr. Thompson,) I descended the pit for the purpose of the experiments on Saturday the 15th of November, at 11 A.M., and remained nearly four hours underground, with the following gentlemen, viz. Mr. W. L. Wharton, Professor Whitley, Rev. Prof. Chevallier, Prof. Johnston, Mr. G. C. Atkinson, and several others.

Barometers and thermometers were taken by Mr. Wharton and myself, and each of the other gentlemen was provided with a thermometer. On a comparison of thermometers from 47° to 70°, it was found that Mr. Whitley's (by Adie) agreed with one constructed by myself. By this standard, all the observations are recorded, proper corrections being applied to the other instruments.

4. The pit is 12 feet in diameter, partitioned in the usual



Seam of coal at 264 fathoms.

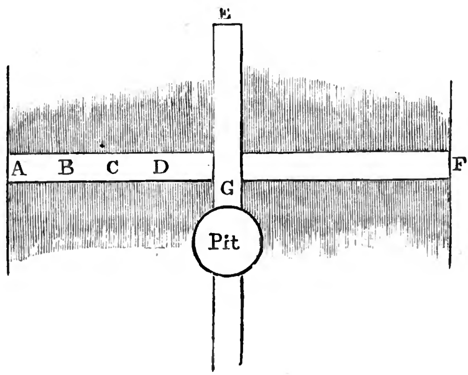
Bottom of Pit.

manner, and *tubbed* in parts to stop the entrance of water,

which nevertheless drops, but not in great quantity, down the sides. This water falls *several yards below the coal seam* in which our experiments took place, into the bottom of the pit, which is to be carried considerably deeper to a lower bed of coal.

On arriving at the point *a*, we entered on the nearly level floor of the coal, and found four narrow headings driven short distances E., W., N., and S. Strong currents of air were established along the headings to sweep the fresh face of coal, from all points of which the carburetted hydrogen gas was heard to issue with a faint tinkling or humming noise, like that which might arise from the vibration of innumerable very small kettles. This gas was carried off too rapidly by the currents of air to permit any exhibition of phænomena depending on its inflammable properties, even when the Davy-lamp was placed in angles of the front of the coal.

By the care of Mr. Foster, the intelligent viewer, a hole was drilled in the floor of the coal seam on the dip side (east) to the depth of two feet; and when Mr. Wharton and I reached the seam, this hole was completed, and full of salt water. This station is marked A on the accompanying plan; it is 22 yards from the pit.



5. The first experiment was made by my going instantly to the bore-hole A, and trying the temperature of the water which had risen in it. By most careful observations it was found to be 70·1. The air in the drift was generally 62°; this was the temperature of the air in the porch G: it prevailed also along the drift E, in which no one was at work, till near the *forehead*, when it rose to 64°, and *close to the coal among the humming* gas, which would not fire, it was 68°.

Each of the thermometers belonging to the party was plunged in the bore-hole A, and the temperature was uniformly found about 70°, allowance being made for the difference of the graduation of the instruments.

6. Mr. Wharton now caused excavations to be made in the floor of the east drift to the depth of about nine inches, and quickly inserted in each a minimum thermometer, previously heated to about 75°—80°, and left them for half an hour. The temperature of the air near the surface of the floor at each point was 62° to 63°; that of the loose small coal on the surface 64° to 67°. It was found upon two trials that at the point D, nearest the pit, the temperature at this depth was 64°·5; at C, further in, 66°·4; at B, six yards from the forehead, 69°·4 and 69°·9. We then chose a point near A, within two or three yards of the forehead, and sunk the instrument deeper, so that it was quickly surrounded by the gushing salt water: when taken up it stood at 71°·4.

7. We next determined to try the temperature of a fresh face of coal, by picking off a few inches in depth, and placing the thermometer in contact with the fresh face and its bulb among the particles which had fallen. The first rough trial gave 69°; the next a temperature continually rising till it settled at $71\frac{1}{4}$°.

8. In trying again the temperature of the bore-hole A, we observed that the thermometer fluctuated through a full degree, being highest whenever bubbles of gas rose more rapidly through the water, and lowest when these ceased for a while. It was also found that this water, notwithstanding the effect of human bodies, lights, &c., was continually *cooling* by the current of air in the drift. At 2 P.M. its temperature was 69°·7 when air-bubbles rose in abundance, and 69°·1 when this was not the case. Some time later it was again registered 69°·6 and 69°·3.

Finding this to be uniformly the case, we returned to the little pit near A, and found by a delicate thermometer the temperature of the water when bubbles abounded to be 72°·6, and 71°·6 and 72°· when they ceased.

9. While these experiments were in progress, Mr. Whitley caused a hole two feet six inches in depth, to be made in the floor of the coal in the forehead of the west or rise drift F, and in this he poured water at 56° Fahr., and then with particular precautions introduced a thermometer at 2 P.M. The men were directed to abstain from entering this drift, till, on Monday morning, after an interval of two days, Mr. Whitley returned, and with Mr. G. Atkinson, drew out the thermometer and found it to stand at 71°·2. As Mr. Whitley has

made arrangements for repeating this interesting experiment, under different conditions, I shall not further anticipate the account, which I hope he will soon publish, of the result of his labours.

10. The temperature of the water collected in the pit bottom was 67°. The barometric observations were:

		B. P.	Att.Th.	Det. Th.	
At the top .... at	11 A.M.	30·518	53°	49°	It is a boiled tube absolutely free from air.
At the bottom at	11 A.M.	32·280	58	62	
At the top .... at	3 P.M.	30·518	58	49	

Capacity of instrument $\frac{1}{21}$.

11. On these results I beg leave to offer a few remarks.

The temperature of surfaces of rock exposed in subterranean situations is subject to several modifying causes.

1. The *real* interior temperature of the mass;
2. The *local* influence of external heating agents, which are, respiration of workmen, lights, explosions, and friction;
3. The *variable* influence of the air which is made to circulate through the passages of the works.

12. It is evident that the most satisfactory experiments are those in which the local and variable influences are the least possible, or else the most easily ascertainable: they cannot be wholly annihilated. In the present instance the small extent of the underground workings is very favourable; they are, in fact, only just commenced; no horses are yet let down into the pit; the extrication of gas from the fresh coal is so abundant as to compel the use of the Davy-lamp, and to require a very powerful ventilation. It is a circumstance too important to be overlooked, that the total influence of the external agents on temperature is the resultant of the heating and cooling agency of the men, lights, currents of fresh air, &c. If this total influence, in any part of the mine, be to augment the temperature of the surfaces, the interior parts will be found colder than the surface, and the contrary.

Now in the present instance, it was found that the resultant of the external influences on the surface of the coal was powerfully refrigerating; for in those parts which were continually and had been long exposed to them, the surface temperature was either the same or a few degrees above that of the air; in the parts which had been less exposed, this temperature was higher: when fresh surfaces were exposed it was highest of all, being 9° 6′ above that of the air current. As the workings proceed this great difference will probably diminish, and then it may be not possible so clearly to prove

the important truth, that *the hottest part is that furthest removed from external modifying causes.*

13. The salt water springing up in the floor of the coal, on the dip side, is a favourable circumstance. Its temperature, like that of the fresh rock, is highest when first tried: by exposure to the external agent it is cooled nearly a degree in three hours, from a temperature which was already nearly 2° too low.

14. The gas, which is constantly bubbling out from its cellular reservoirs, brings more completely, perhaps, than anything else, the real temperature of the interior. It no doubt supplies warmth to the surface of the coal, and was found hotter than the water through which it rose by no less than a degree.

15. Mr. Whitley's experiment is surely liable to no objection but one, viz. that the pouring of water at 56° into so bad a conductor as coal, was likely to give a result rather below the truth.

16. I have said nothing of local chemical action, since in this situation no indications of oxides or salts of iron, or other products, depending on decomposition of pyrites, or other probable sources, have as yet manifested themselves. Besides this, all the deep coal-works of this country, as I learn from Mr. Buddle, agree in proving the augmentation of temperature to be a general fact, independent of the pure or pyritous quality of the coal, and of the development of inflammable gas.

17. The total depth of the floor of the coal is 1584 feet; the pit top is 87 feet above ordinary high water; its depth below the level of the sea, 1497 feet. If the depth of the invariable plane be taken at 100 feet, and the mean temperature of the place 47°·6, we have 72°·6 —47°·6 = 25°·0 augmentation of temperature in 1484 feet = 1° for 59·36 feet, or in round numbers 1° Fahr. for 20 yards English.

Newcastle, Nov. 18, 1834.

LXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. iv. p. 441.]

1834. May 15.—A PAPER was read, entitled, "Of the Functions of some parts of the Brain; and of the relations between the Brain and Nerves of Motion and Sensation." By Sir Charles Bell, K.H., F.R.S.

The author commences his paper by an enumeration of some of the sources of difficulty and of error which have impeded the progress of

discovery in the physiology of the brain; the first impediment to which, he observes, "is in the nature of the inquiry, since extraordinary and contradictory results must be expected from experimenting on an organ so fine as that must be which ministers to sensibility and motion, and which is subject to change on every impression conveyed through the senses." Another cause of fallacy is the dependence of the brain on the condition of the circulation within it: but the most frequent source of error is the obscurity which hangs over the whole subject; for although the brain be divided naturally into distinct masses, not one of these grand divisions has yet been distinguished by its functions; and hence we may account for the failure of all attempts to explain the phenomena which attend injury of the brain. The principle, now universally admitted, that nerves have distinct functions, and not a common quality, is pursued by the author in his investigation of the structure of the brain, in which he follows the nerves into that organ, and observes the tracts of nervous matter from which they take their origin. He concludes from his inquiries that both sensibility and motion belong to the cerebrum; that two columns descend from each hemisphere; that one of these, the anterior, gives origin to the anterior roots of the spinal nerves, and is dedicated to voluntary motion; and that the other, which from its internal position is less known, gives origin to the posterior roots of the spinal nerves, and to the sensitive root of the fifth nerve, and is the column for sensation. He further shows that the columns for motion, which come from different sides of the cerebrum, join and decussate in the medulla oblongata; that the columns of sensation also join and decussate in the medulla oblongata; and lastly, that these anterior and posterior columns bear, in every circumstance, a very close resemblance to one another, in as much as the sensorial expansions of both are widely extended in the hemispheres; for they pass through similar bodies towards the base of the brain, and both concentrate and decussate in the same manner; thus agreeing in every respect, except in the nervous filaments to which they give origin. Hence he explains the phenomena of the loss of sensibility as well as the power of motion of one side of the body, consequent on injuries of the other side of the brain.

The Society then adjourned over Whitsun Week to the 29th of May.

May 29.—A paper was read, entitled, "On the Principle of Construction and General Application of the Negative Achromatic Lens to Telescopes and Eyepieces of every description." By Peter Barlow, Esq., F.R.S.

This paper is intended as a more full illustration of the principles on which the negative achromatic lens is constructed and applied, than has been given in the extract from the author's letter to Mr. Dollond, contained in the paper of the latter, lately read to the Society, on his ingenious application of that lens to the micrometer eyepiece*. The author shows that its advantages are not confined to this instrument,

* An abstract of Mr. Dollond's paper appeared in Lond. and Edinb. Phil. Mag., vol. iv. p. 364.

but that it is applicable to any eyepiece positive or negative to the erecting eyepiece, and, indeed, to any telescope of fluid or glass, and also to refractors.

A paper was also read, entitled, "Some remarks in reply to Dr. Daubeny's Note on the Air disengaged from the Sea over the site of the recent Volcano in the Mediterranean." By John Davy, M.D., F.R.S. Assistant Inspector of Army Hospitals.

Respecting the air in question, which Dr. Davy had found to consist of about 80 per cent. of azote and 10 oxygen, he had remarked that two views might be taken of its origin; the one, that it was of volcanic source; the other, that it was derived from the sea water, and merely disengaged by the heat of the volcano. Dr. Davy, rejecting the former of these views, had adopted the latter, for reasons, the validity of which was controverted by Dr. Daubeny*; and the purpose of the present paper is to answer the objections urged against them, and to bring additional evidence in support of his opinion.

A paper was then read, entitled, "On the number of Primitive Colorific Rays into which White Light may be separated." By Paul Cooper, Esq. Communicated by J. G. Children, Esq. Sec. R.S.

From a consideration of the circumstances in which white light is decomposed by the prism, in different experiments, and of the various appearances of the spectra which result, the author is led to the opinion that the primary colours composing white light are not seven, as conceived by Newton; nor four, as supposed by Wollaston; but only three: and that these three are not red, yellow, and blue, as imagined by Brewster, but red, green, and violet; the first and last forming the terminal parts of the spectrum, and the green occupying an intermediate position; and the various tints which intervene being the result of superpositions, in various quantities, of these respective primary colours. He pursues the consequences of this hypothesis, applying it to a great variety of forms of experiment, not only by the direct observation of beams of refracted light, but by viewing the prismatic spectrum through different media, capable of absorbing each of the primitive colours in different degrees: and he finds the results to accord exactly with the hypothesis he proposes, and on which he therefore concludes that their true explanation must be founded. He conceives that the errors of preceding experimentalists have arisen from their neglecting to take into account the effects of diffraction, which introduces considerable confusion into the results.

A paper was also read, entitled, "An Investigation of the Laws which govern the Motion of Steam-Vessels, deduced from experiment." By P. W. Barlow, Esq. Civil Engineer. Communicated by Dr. Roget, Sec. R.S.

The author commences with the description of a paddle-wheel for steam-vessels, of a new construction, in which the floats are made to enter and leave the water nearly in a vertical position. He then investigates several formulæ adapted to the calculation of the forces and

* An abstract of Dr. Daubeny's note was given in vol. iii. p. 447.

velocities arising from this form of the apparatus; and gives an account of the results of various experiments made on its efficiency as compared with the common wheels, and with relation to the consumption of fuel. The general results to which he is led are as follow:—1st. When vessels are so laden as that the wheel is but slightly immersed, little advantage is derived from the vertically acting paddles. 2ndly. In cases of deep immersion, the latter has considerable advantage over the wheel of the usual construction. 3rdly. In the common wheel, while the paddle passes through the lower portion of the arc, that is when its position is vertical, it not only affords less resistance to the engine, but is less effective in propelling the vessel than in any part of its revolution. 4thly. The paddle of the wheel, while passing through the lower portion of the arc, affords more resistance to the engine, and is more effective in propelling the vessel, than in any part of its revolution; a property which is a serious deduction from its value; for, in consequence of the total resistance to all the paddles being so much less than in the common wheel, much greater velocity is required to obtain the requisite pressure, and a greater expenditure of steam power is incurred. This loss of power is most sensible when the wheel is slightly immersed; but in cases of deep immersion the vertical paddle has greatly the advantage. 5thly. In any wheel, the larger the paddles the less is the loss of force; because the velocity of the wheel is not required to exceed that of the vessel in so great a degree, in order to acquire the resistance necessary to propel the vessel. 6thly. With the same boat and the same wheel no advantage is gained by reducing the paddle so as to bring out the full power of the engine; the effect produced being simply that of increasing the speed of the wheel, and not that of the vessel. 7thly. An increase of speed will be obtained by reducing the diameter of the wheel; at least within such limits as allow of the floats remaining sufficiently immersed in the water; and provided the velocity of the engine does not exceed that at which it can perform its work properly. 8thly. An advantage would be gained by giving to the wheel a larger diameter, as far as the immersion of the paddles produced by loading the vessel would not so sensibly affect the angle of inclination of the paddle; but this advantage cannot be obtained with an engine of the same length of stroke, because in order to allow the engine to make its full number of strokes, it will then be necessary to diminish the size of the paddles, which is a much greater evil than having a wheel of smaller diameter with larger paddles.

The reading of a paper was then commenced, entitled, "On the Equilibrium of a Mass of Homogeneous Fluid at liberty." By James Ivory, Esq., K.H., M.A., F.R.S.

June 5.—Mr. Ivory's paper, entitled, "On the Equilibrium of a Mass of Homogeneous Fluid at liberty," was resumed and concluded.

The author shows that Clairaut's theory of the equilibrium of fluids, however seductive by its conciseness and neatness, and by the skill displayed in its analytical construction, is yet insufficient to solve the problem in all its generality. The equations of the upper surface

of the fluid, and of all the level surfaces underneath it, are derived, in that theory, from the single expression of the hydrostatic pressure, and are entirely dependent on the differential equation of the surface. They require, therefore, that this latter equation be determinate and explicitly given; and accordingly they are sufficient to solve the problem when the forces are known algebraical expressions of the co-ordinates of the point of action; but they are not sufficient when the forces are not explicitly given, but depend, as they do in the case of a homogeneous planet, on the assumed figure of the fluid. In this latter case, the solution of the problem requires, further, that the equations be brought to a determinate form by eliminating all that varies with the unknown figure of the fluid; and the means of doing this are not provided for in the theory of Clairaut, which tacitly assumes that the forces urging the interior particles are derived from the forces at the upper surface, merely by changing the co-ordinates at the point of action. In the case of a homogeneous planet, the forces acting on the interior particles are not deducible, in the manner supposed, from the forces at the surface.

After showing that the equilibrium of a fluid, entirely at liberty, will not be disturbed by a pressure of the same intensity applied to all the parts of the exterior surface, the author considers the action of the forces upon the particles in the interior parts of the body of the fluid; and shows that although the forces at the surface are universally deducible from the general expressions of the forces of the interior parts, yet the converse of this proposition is not universally true, the former not being always deducible from the latter; a distinction which is not attended to in Clairaut's theory. He then investigates the manner in which these two classes of forces are connected together; establishes a general theorem on the subject; and proceeds to its application to some of the principal problems, relating to the equilibrium of a homogeneous fluid at liberty, and of which the particles attract one another with forces, first in the inverse duplicate ratio, and secondly in the direct ratio of the distance, at the same time that they are urged by a centrifugal force arising from their revolution round an axis. The author concludes with some remarks on Maclaurin's demonstration of the equilibrium of the oblate elliptical spheroid; and on the method of investigation followed in the paper published in the Philosophical Transactions for 1824*. In an Appendix the author subjoins some remarks on the manner in which this subject has been treated by M. Poisson.

The reading of a paper was then commenced, entitled, "Experimental Researches in Electricity;" Eighth Series." By Michael Faraday, Esq., D.C.L., F.R.S.

June 12.—A paper was read, entitled, "On the Arcs of certain Parabolic Curves." By Henry Fox Talbot, Esq., M.P., F.R.S.

The general equation to parabolic curves, (namely, $nu = v^n$; where u is the abscissa and v the ordinate,) gives for the arc of the curve an

* A paper by Mr. Ivory also relating to this subject will be found in Phil. Mag., vol. lxiii. pp. 339, 347.

expression which, excepting in a very few instances, is transcendental. But although the length of an arc, considered by itself, cannot be assigned algebraically, yet it frequently happens that the sum of two or more arcs is capable of being so assigned, and sometimes in a very simple manner. The author has found this reduction to take place in so many instances, as to incline him to believe that it may be universally possible, provided the exponent (n) of the ordinate in the equation to the curve is a rational quantity of these reductions: he gives a great number of examples; but although the processes for that purpose are easy, the difficulty consists wholly in finding the proper method of treating each individual case. The author hopes to lay before the Society, on a future occasion, an account of the principles on which this branch of analysis is founded.

Mr. Faraday's Eighth Series of Experimental Researches in Electricity was resumed and read in continuation.

June 19.—The reading of Mr. Faraday's Eighth Series of Experimental Researches in Electricity was resumed and concluded.

This series is devoted to an investigation of the source, character and conditions of the electricity of the voltaic instrument, and is divided into five parts. In the first part, simple voltaic circles are considered; and at the outset, the great question of "whether the electricity is due to contact or chemical action?" is investigated and decided by apparently very conclusive evidence in favour of the latter. One principal experiment in favour of this decision is the following: A plate of zinc and a plate of platina were prepared; one end of each of these was put into a vessel containing a little dilute sulphuric acid or sulpho-nitric acid, and between the other ends was placed a piece of bibulous paper moistened in a solution of iodide of potassium: the two plates did not touch each other anywhere, but still the action of the acid at the one extremity was able to induce the electro-chemical decomposition of the iodide of potassium at the other. That this decomposition was due to the chemical action of the acid was proved by removing the latter; for then the decomposition ceased. It was also further proved by the appearance of the iodine against the *platina*; for it went there in consequence of the passage of a current (induced by the action of the acid) having the opposite direction to that which the solution of iodide would have produced had it been the only exciting body, and had metallic contact been allowed.

The opposition of the chemical affinities at the two places where the acid and the solution of the iodide are placed, is shown when the metal plates are allowed to touch each other in the middle; for then two opposite electric currents are produced, but that occasioned by the acid is the stronger. This opposition is further shown in the manner in which the weaker set of affinities are overcome by the stronger (that is, those of the iodide and zinc by those of the acid and zinc); and this dependence and relation of the two explains at once the value of metallic contact; for if the solution of iodide of potassium be placed between platina and platina, one of those pieces of metal touching the zinc which is immersed in the acid, then the solution of iodide does not tend to throw an electric current into circulation,

because it exerts no chemical action in either direction; and therefore the powers active in the acid are more free to act, produce a stronger current, and effect decomposition more freely.

The chemical actions at the opposite ends of the metallic arrangement are so strongly associated and related, that in the most perfect form of experiment, action cannot occur at either end without also taking place at the other extremity to an exactly equivalent amount. This is considered by the author as the most convincing proof that in the voltaic pile the chemical and electric action are the same; that is, modes of exhibition of the same force, and as they are convertible into each other in exactly definite proportion, must have one common origin.

By using different fluids at the exciting place of action, currents of different intensity could be obtained: thus the current produced by the action of dilute sulphuric acid on zinc and platina could decompose elsewhere solution of iodide of potassium, fused protochloride of tin, or chloride of silver; but could not decompose water, muriatic acid, nitric acid, or the chloride or iodide of lead. Making the dilute sulphuric acid stronger, or using larger plates of zinc and platina, did not yield any advantage; but immediately that the chemical action on the zinc was increased in *intensity*, which could be done by adding only a few drops of nitric acid, then most of the latter bodies could be decomposed by a single pair of plates. A scale of initial intensities can in this way be obtained.

The electricity evolved in the voltaic pile is altogether due to that chemical action which takes place between the metal most easily acted upon and the element which unites with it; as, for instance, between the zinc and the oxygen of the water, or the chlorine of the muriatic acid, or the sulphur of hydrosulphurets, &c.; the after action of the acid in combining with the oxide, when that is the substance formed, adds nothing to the effect. The truth of this principle is deduced in the first place from the electricity evolved being the equivalent of the zinc oxidized; in the second, from the quantity of electricity being the same for the oxidation of a given quantity of zinc, whether the oxide formed is removed by an acid or an alkali; and it is supported by many other experimental reasons and proofs.

The view which the author takes of the identity of electrical and chemical action, leads him to admit that there are *two modes of action* in which the attractive power of the substances which ultimately combine, and by combining give the voltaic pile activity, can be exerted. Thus, taking zinc and platina as the two metals used, then the *third* substance must be an electrolyte; that is, a body which is decomposed when the electric current passes it; which cannot conduct the current unless it is at the same time decomposed; and which contains an element having such attraction for the zinc that the latter can take it from the element with which it is previously combined. Water is the electrolyte generally present in the voltaic pile.

Then, with respect to the attraction between the zinc and the oxygen of the water, we have it in our power to cause it to take place at once when the metal and water are in contact, the hydrogen being then set free; or we can, by using the precautions which the author

gives, cause that no action take place, unless a current be formed and the hydrogen be transferred to a distance, whilst the forces circulate in what is called the electric current. Placing the origin of the current in the chemical action, which yet could be thus virtually restrained unless the circuit was completed, the author expected to find a state of tension in the chemical or electrical forces *before* metallic contact was made or the circuit perfect, and was able at last to prove this most fully by obtaining an *electric spark* between two plates of different metals immersed in acid before they came in contact. This fact, with the former one of decomposition, fully proved that contact was not necessary to the production of the electricity in the voltaic pile.

The *second* part of this memoir contains an investigation of the following important points: namely, whether electrolytes could resist the action of an electric current if below a certain intensity; whether the intensity at which an electric current might cease to act would be the same for all bodies; and also whether the electrolytes, when thus resisting decomposition, would conduct the electricity as a metal or charcoal does, after they ceased to conduct as electrolytes, or would act as insulators. It is first proved with regard to water, that a current of a certain intensity is necessary for its decomposition, but that a current of a lower intensity is conducted by it; and that with such feeble currents, pure water conducts as well as acidulated water or saline solutions. The same condition of a certain necessary intensity of current was found to hold good also with sulphate of soda in solution, with fused chloride of lead and other bodies, and is considered by analogy as extending to all electrolytes.

In the *third* part of the paper, associated voltaic circles, or the voltaic battery, is examined. From the principles and facts stated in the preceding parts, it appears evident that the association of many pairs of plates, equal in size, nature and force, cannot by any possibility increase the quantity of electricity above that which any single pair in the series could produce, taking the quantity of zinc oxidized at any one plate as the standard of development. It is easy, by using amalgamated zinc, to construct a battery in which no action shall take place on the metals, except the extremities be in communication. If a battery of ten pairs of plates be thus communicated, there is of course oxidation of each zinc plate, and a current of electricity circulates. If the contact of the extremities be continued until a certain quantity of zinc has been dissolved at any one plate, it will be found that an exactly equal quantity has been dissolved at each of the other plates; and that a certain quantity of electricity has passed, which can be taken cognizance of by the volta-electrometer. But should nine of the pairs of plates be removed and the battery be reduced to a single pair, yet when the given quantity of zinc had been dissolved there, as much electricity would have gone round the circuit as with the whole number of ten pairs, and during the evolution of which ten times the quantity of zinc had been oxidized.

This result, already proved by electro-magnetic experiments, is shown to be a necessary consequence of the construction of the pile and the manner in which its forces act. The electricity evolved by

chemical action at one pair of plates cannot pass by another pair except an equal chemical action take place there; and as the chemical and electrical action are always equivalent, the equal chemical action at the second pair will do no more than suffice to transfer forwards the forces disturbed at the first pair, and can add nothing to their quantity: but they can add to their *intensity*, and in fact the recurrence of a second chemical action at the second pair of plates has exactly the same effect as would be produced by a more intense chemical action at the first pair. In this way it is that numbers of plates give energy to the voltaic pile, and enable its power to penetrate electrolytic bodies and permeate bad conductors in a manner which could not be done by the electricity of a few pairs of plates only.

The *fourth* part of the paper relates to the resistance opposed to the electric current at the place of decomposition, and refers this at once to the resistance of the chemical affinity which has to be overcome. This of course varies with the number of places where decomposition is effected, the strength of the affinity of the elements of the decomposing body for each other, and the nature of the substance against which the decomposition is effected, and by which it may very frequently be assisted. All these are taken into account, their general, and occasionally particular, results shown, and their perfect harmony with the principles previously advanced pointed out.

In the last part of the paper some general remarks on the active voltaic battery are made, in which the influence of several distinct causes in producing a rapid change and deterioration of action is pointed out. Each of these causes is considered separately, and the effects they produce are shown to be necessary consequences of the principles already laid down as those of the voltaic battery.

GEOLOGICAL SOCIETY.

Nov. 5, 1834.—The Society assembled this evening for the Session. A paper was read "On a new Classification of Fishes, and on the Geological Distribution of Fossil Fishes," by Prof. Agassiz, of Neuchatel.

The author begins by stating that the state of the science of Ichthyology had obliged him to undertake an examination of recent fish for the sake of comparing them with the fossil species, and in doing so he had arrived at a classification of fish, in general, differing considerably from the various arrangements previously adopted by naturalists. One of the essential characters of fish is, to have their skin covered with scales of a peculiar form and structure. This covering, which protects the animal without, is in direct relation with its internal organization, and Dr. Agassiz has found that by an attentive examination of the scales, fish may be divided into more natural orders than had hitherto been established. In this manner he has established four orders, which bear some relation to the divisions of Artedi and Cuvier, but one of which, hitherto completely misunderstood, is almost exclusively composed of genera whose species are only found in the most ancient strata in the crust of our globe. These four orders are, the *Placoidians*, which comprehend the cartilaginous fish of Cuvier, with the exception of the sturgeon; the

Ganoidians, which comprehend above fifty extinct genera, and to which we must refer the Plectognaths, Syngnathus, and Acipenser; thirdly, the *Ctenoidians*, which are the Acanthopterygians of Cuvier and Artedi, with the exception, however, of those which have smooth scales, and with the addition of the Pleuronectes. Lastly, the *Cycloidians*, which are principally Malacopterygians, but which comprehend besides all those families excluded from the Acanthopterygians of Cuvier, and from which we must take the Pleuronectes already placed in the preceding order.

If we estimate the number of fish now known to amount to about eight thousand, we may state that more than three fourths of this number belong to two only of the above-mentioned orders; namely, the Cycloidians and Ctenoidians, whose presence has not yet been discovered in the formations inferior to the chalk. The other fourth part of living species is referrible to the orders Placoidians and Ganoidians, which are now far from numerous, but which existed during the whole period which elapsed since the earth began to be inhabited, to the time when the animals of the greensand lived. These remarkable conclusions to which M. Agassiz had come, from the study of more than six hundred fossils on the Continent, have been corroborated by the inspection of more than two hundred and fifty new species found in English collections.

The author next observes that in the class of fish more considerable differences may be remarked within narrow geological limits than among inferior animals. We do not see in the class of Fishes the same genera, nor even the same families, pervading the whole series of formations as takes place among Zoophytes and Testacea. On the contrary, from one formation to another, this class is represented by very different genera, referable to families which soon become extinct, as if the complicated structure of a superior organization could not be long perpetuated without important modifications; or rather, as if animal life tended to a more rapid diversification in the superior orders of the animal kingdom, during equal periods of time, than in its lower grades. With respect to this, it is with fish nearly as with mammifers and reptiles, whose species, for the most part but little extended, belong at a short distance in the vertical series to different genera, without passing insensibly from one formation to another, as is generally admitted to be the case with certain shells. One of the most interesting facts which Mr. Agassiz has observed is, that he does not know a single species of fossil fish which is found successively in two formations, whilst he is acquainted with a great number which have a very considerable horizontal extent. But the class of Fish presents besides to Zoological Geology, the immense advantage of traversing all formations. Thus they afford us the only example of a great division of vertebrated animals in which we may follow all the changes experienced in their organization during the greatest lapse of time of which we possess any relative measure.

The fish of the tertiary formations approach nearest to recent fish, yet hitherto the author has not found a single species which he con-

siders perfectly identical with those of our seas, except the little fish which is found in Greenland in geodes of clay, and whose geological age is unknown to him.

The species of the crag of Norfolk, the superior subapennine formation, and the molasse, are related for the most part to genera now common in tropical seas; such are the Platax, the large Carcharias, the Myliobates, with large palatal plates, and others. In the inferior tertiary formations, the London clay, the calcaire grossier of Paris, and Monte Bolca, a third at least of the species belong to *genera* which exist no longer. The chalk has more than two thirds of its species referrible to *genera* which have now entirely disappeared. In it we already even see some of those singular forms which prevail in the Jurassic series. But as a whole the fish of the chalk recall more forcibly the general character of the tertiary fish than that of the species of the Jurassic series.

If we paid attention only to fossil fish in the grouping of geological formations on a large scale, the author thinks it would be more natural to associate the cretaceous with the tertiary strata than to place the former among the secondary groups. Below the chalk there is not a single genus which contains recent species, and even those of the chalk which have them contain a much greater proportion of species which are only known as fossil. The oolitic series, to the lias inclusive, forms a very natural and well defined-group, in which also must be included the Wealden, in which Mr. Agassiz states he has not found a single species referrible even to the genera of the chalk. Henceforth, the two orders which prevail in the present creation are found no more; whilst those which are in a small minority in our days, appear suddenly in great numbers. Of the Ganoidians, those genera which have a symmetrical caudal fin are found here, and among the Placoidians those above all predominate which have their teeth furrowed on both the external and internal surface, and have large thorny rays. For it is now certain that those great rays which have been called Ichthyodorulites, belong neither to Silures nor Balistæ, but are the rays of the dorsal fin of the great Squaloids, whose teeth are found in the same strata.

On leaving the lias to come to the inferior formations, we observe a great difference in the form of the posterior extremity of the body in the Ganoidians. All have their vertebral column prolonged at its extremity into a single lobe, which reaches to the end of the caudal fin, and this peculiarity extends even to the most ancient fish. Another observation worthy of attention is, that we do not find fish decidedly carnivorous before the carboniferous series; that is to say, fish provided with large conical and pointed teeth. The other fish of the secondary series before the chalk appear to have been omnivorous, their teeth being either rounded, or in obtuse cones, or like a brush.

The discovery of coprolites containing very perfect scales of fish which had been eaten, permits us to recognise the organized beings which formed the food of many ancient fish; even the intestines, and in some fossil fish of the chalk the whole stomach is preserved, with its different membranes. In a great number of fish from Shep-

pey, the chalk, and the oolite series, the capsule of the bulb of the eye is still uninjured; and in many species from Monte Bolca, Solenhofen, and the lias, we see distinctly all the little blades which form the branchiæ.

It is in the series of deposits below the lias that we begin to find the largest of those enormous sauroid fish whose osteology recalls, in many respects, the skeletons of saurians, both by the closer sutures of the bones of the skull, their large conical teeth, striated longitudinally, and the manner in which the spinous processes are articulated with the body of the vertebræ and the ribs at the extremity of the spinous processes.

The small number of fish yet known in the transition formations does not as yet permit the author to assign to them a peculiar character, nor has he discovered in the fossil fish of strata below the greensand any differences corresponding with those now observed between marine and freshwater fish, so that he cannot, on ichthyological data, decide on the freshwater or marine origin of the ancient groups.

This paper is accompanied by Tables of the fossil fish of different formations.

LXVI. *Reviews, and Notices respecting New Books.*

The Transactions of the Entomological Society of London. Vol. I. Part I. *With Seven Plates.* 8vo. pp. 108.

WE are here presented with the first publication of a Society established for the cultivation of a department of Natural History which is fast gaining ground amongst us. It would be out of place here to enlarge upon the interest and importance of the study of that vast portion of animated nature known by the general term Insects; suffice it to say that this First Part of the Transactions of the Entomological Society, in the prospectus of its Prize Essays, as well as in several of the Communications, gives evidence of the practical utility to which the attention of the Society is directed; and which, if persevered in, cannot fail of establishing it on the firmest basis.

Passing over the Introduction, which mentions the origin of the Society, and comments, perhaps in rather too severe terms, upon the opposition which has been raised against the publication of its Transactions, we are presented with a series of thirteen memoirs, which from their varied character cannot fail to interest not only the professed entomologist but also the general reader. Of the latter character may be mentioned Mr. Spence's "Observations on a Mode practised in Italy of excluding the common House-fly from Apartments;" Mr. W. B. Spence's Remarks on a passage in Herodotus, relative to the attacks of Gnats, which has greatly perplexed the Commentators; Mr. W. E. Shuckard's Observations upon the habits of the indigenous Sand Wasps; valuable not only on account of the facts narrated, but from the correction thereby afforded of a theory recently proposed relative to these insects by M. le Comte de Saint Fargeau; Mr. W. W. Saunders's Paper upon the habits of some Indian Insects; and Mr. Lewis's "Explanation

of the sudden Appearance of the Web-spinning Blight of the Apple, Hawthorn, &c." Among those of a more purely scientific character may be mentioned: the Rev. F. W. Hope's Descriptions of some hitherto uncharacterized exotic Coleoptera, chiefly from New Holland; Mr. Waterhouse's Descriptions of the Larvæ and Pupæ of various Species of Beetles; Mr. Lewis's Descriptions of some new Genera of British Homoptera; Mr. G. R. Gray's Descriptions of several Species of Australian Phasmata, &c.; whilst others are of a mixed nature, as Mr. Christy's Remarks on a Species of Calandra occurring in the Stones of Tamarinds; Mr. Waterhouse's Account of *Raphidia ophiopsis*; and Mr. Westwood's Description of the Nest of a gregarious Species of Butterfly from Mexico. The part concludes with the Journal of Proceedings, (a judicious addition, we think, and almost new in the "Transactions" of British societies,) By-Laws, and List of Members. The plates are beautifully executed, each containing a great amount or variety of subjects.

LXVII. *Intelligence and Miscellaneous Articles.*

DISCOVERY OF SAURIAN BONES IN THE MAGNESIAN CONGLOMERATE NEAR BRISTOL.

ALTHOUGH some of the earliest noticed Saurian remains were the fossil Monitors of Thuringia, discovered in the Continental equivalents of our magnesian limestone,—characterized by the same testacea and fishes which Mr. Sedgwick has so fully described in our own corresponding formations in the North of England,—it does not appear that Saurian remains have been until now detected in this geological site in our own series. Recently, however, a quarry of the magnesian conglomerate, resting on the highly inclined strata of carboniferous limestone, at Durdham Down, near Bristol, has afforded some Saurian vertebræ, ribs, femora, and phalanges, together with claws, the latter of considerable proportional size: a coracoid bone has also been found, approaching very nearly to that of the Megalosaurus. The general character of the bones seems intermediate between those of this genus and the crocodile. Dr. Riley, who submitted the specimens hitherto discovered, to the Literary and Philosophical Society of the Bristol Institution, is understood to be preparing a detailed account of this interesting discovery for the Geological Society.

The only Saurian remain hitherto found in this island in a site approaching to this, was a fragment of a lower jaw apparently of a Gavial discovered in the lower beds of the new red sandstone at Guy's Cliff, Warwickshire. This fact was communicated by Mr. Conybeare to Mr. Parkinson, and is noticed in the abridged work of the latter on Organic Remains.

EXPERIMENTS ON THE ACTIVE PRINCIPLE OF SARSAPARILLA. BY M. POGGIALE.

M. Palotta, in 1824, announced an active principle in sarsaparilla which he called *parilline*. Soon afterwards M. Folchi thought he had discovered a new principle which he named *smilacine*. In the

year 1831, M. Thubeuf stated that he had extracted a new substance from sarsaparilla to which he gave the name of *salseparine*, and lastly, M. Batka, a German chemist, towards the end of the year 1833, described an acid which he called *parillinic acid*. The question to be resolved is, are these four substances four new bodies, or are they merely one and the same principle obtained by different methods?

A considerable quantity of parilline, smilacine, salseparine, and parillinic acid was prepared; following the process of M. Palotta, very fine parilline was obtained, but the smilacine of M. Folchi was not so easily prepared; and he was undoubtedly mistaken in saying that an appreciable quantity of smilacine might be obtained by macerating in water one ounce of the pith of sarsaparilla, treating the solution with animal charcoal, and evaporating. It is, according to M. Poggiale, impossible to extract from one ounce of the pith, by means of water, the smallest portion of smilacine; the method adopted by him was to treat the pith with alcohol and charcoal, and the product possessed all the properties of parilline. Although M. Thubeuf did not publish his mode of preparing salseparine, the process was known to be that of making an alcoholic solution of sarsaparilla, treating this with animal charcoal and crystallizing it: the substance obtained possessed no properties differing from those of parilline, as will presently be proved. Parillinic acid was also prepared according to the directions of M. Batka.

These four substances are all white, inodorous, and when deprived of water, without taste. They are very bitter and nauseous to the taste when dissolved in water or alcohol. They are heavier than water; insoluble in cold, but slightly soluble in boiling, water; very soluble in boiling alcohol, less so in cold; boiling æther dissolves them all; the volatile oils take them up perfectly; they are less soluble in the fixed oils; they exert but slight action on turmeric paper, none on litmus, and turn the syrup of violets green. They are all decomposed in the same manner by heat, leaving an extremely light charcoal possessing a metallic lustre. They all crystallize in *radiated acicular crystals* when an alcoholic solution is carefully evaporated.

The substance obtained by M. Batka is not an acid; it is true it reddens litmus paper, but this property is owing to a small quantity of muriatic acid which it retains: if, however, this supposed acid is washed seven or eight times with water, it will no longer exert any action on the litmus paper. M. Poggiale attaches the greatest importance to the analyses of these substances, which he states he has performed with the greatest care.

Analysis of Salseparine.

Salseparine, dried at 248° Fahr. in a stove and analysed by M. Liébig's apparatus, gave the following results.

	1st.	2nd.	3rd.
Carbon	62·53	62·39	62·70
Hydrogen	8·80	8·59	8·28
Oxygen	28·67	26·02	29·02
	100·	97·	100·

The Analysis of Parilline by the same apparatus gives

	1st.	2nd.	3rd.
Carbon	62·22	62·99	62·07
Hydrogen	8·96	8·76	8·40
Oxygen	28·82	28·25	29·53
	100·	100·	100·

Analysis of Parillinic Acid.

	1st.	2nd.	3rd.
Carbon	62·98	62·38	62·76
Hydrogen	8·88	8·96	8·63
Oxygen	28·14	28·66	28·61
	100·	100·	100·

Analysis of Smilacine.

	1st.	2nd.	3rd.
Carbon	62·83	62·43	62·08
Hydrogen	8·41	8·68	8·78
Oxygen	28·76	28·89	29·14
	100·	100·	100·

Which experiments denote the atom of salseparine to consist of carbon 8 atoms, hydrogen 15 atoms, and oxygen 3 atoms. From these facts M. Poggiale arrives at the following conclusions: That M. Palotta discovered the active principle of sarsaparilla; that smilacine, parillinic acid, and salseparine are identical with the parilline of M. Palotta, but procured by different methods; that the properties of these four substances are the same; and that the pith of sarsaparilla as well as the bark contains salseparine.—*Journal de Pharmacie*, October 1834.

PREPARATION OF CYANURET OF POTASSIUM.

According to M. Robiquet, when ferrocyanuret of potassium is calcined in a retort, there are formed cyanuret of potassium and quadricarburet of iron; and when the retort is carefully broken, a certain quantity of perfectly pure fused cyanuret of potassium is obtained, which is quite fit for medicinal use.—*Ibid.* Sept. 1834.

COMPOSITION OF LITHIC ACID.

M. Liebig observes that lithic acid is unquestionably one of the most remarkable organic acids, both with respect to its composition and the effects which it produces in certain diseases, such as gravel, urinary calculi, and gouty concretions.

The composition of this acid has been variously stated, and no two analyses agree: these differences arise from the circumstance of chemists not having directly determined the proportion of carbon which the acid contains, and are consequently deprived of the controul in determining the azote which M. Liebig conceives that his apparatus admits of.

The results of the analysis were

	By experiment.	By calculation.		Atomically.
Carbon	36·083	36·11	5 atoms. .	382·185
Azote	33·361	33·36	4 ——	364·072
Hydrogen	2·441	2·34	4 ——	24·959
Oxygen	28·126	27·19	3 ——	300·000
	100·011	99·		1071·216

According to this atomic weight, the compounds of lithic acid with bases which are at present known, are supersalts.—*Ibid.*, p. 571.

GEOLOGICAL SURVEY OF THE UNITED STATES.

We announce with great satisfaction a most important act of legislation by the Congress of the United States, the authorization of a geological survey of that fine country, so rich in minerals and geological phænomena. It gives us pleasure also to add that President Jackson has committed the execution of this arduous undertaking to Mr. Featherstonhaugh, one of the members of the Geological Society of London, who has acquired deserved reputation as a practical and ardent geologist. This gentleman has been many years a resident of the United States, and it is of him that Mr. Conybeare, in his celebrated Report *, says,—" Mr. Featherstonhaugh, a geologist eminently qualified, from his intimate acquaintance with European formations, to advance those comparative views which demand the principal attention in our science."

We cannot but look with unmixed admiration upon the steadiness with which all the great interests of the United States are pursued: the States have wisely concurred in a great act of legislation that cannot but redound to the best interests of their country, and the substantial advancement of natural science. It is an act that Europe will admire, and we hope imitate, and that will ever reflect great honour upon the administration of the present distinguished chief magistrate of the United States.

Private letters state, that Mr. Featherstonhaugh left Washington in July, for the country west of the Mississippi, to examine the high lands in the Arkansa territory, called the Ozark and Mazerne Mountains. These are separated, to the distance of about five hundred miles, by the gorge of the Mississippi valley, from the high lands east of the Mississippi, which constitute the Alleghany ranges; and their geological structure has not yet been examined.

By the kindness of Dr. Buckland we are enabled to annex some extracts from a letter addressed to that eminent Geologist by our friend R. C. Taylor, Esq., F.G.S., dated some time before the Act of Congress, but containing some interesting particulars of the Geology of the United States.

" I am happy to report that the spirit of geological inquiry is fast spreading in this state. A committee is now sitting in the House of Representatives on the subject of a Geological Survey of the whole

* See our last volume, p. 427.

of Pennsylvania, at the expense of the state, for the purpose of constructing a geological map, and of exhibiting and reporting upon all its mineral products and resources. I find my name is nominated to this duty, in conjunction with two gentlemen; one of them, Col. Long, of the U.S. Topographical Engineers, the other, Mr. Tanner, a gentleman connected with the publishing Topographical department. The assent of the Government is fully expected to this important proposition, and if so, it will go into operation very shortly.

"This is a very interesting region, as I hope hereafter to show. Mineralogically it has nothing to boast, except its vast deposits of hæmatitic iron ore, which is dug out exactly in the same manner as gravel out of the gravel pits in England. These accumulations always occur in the limestone valleys. The average yield of iron being about fifty per cent., numerous iron-works are established in these valleys. I say valleys because they are numerous, and parallel to the main Alleghany ridge, and are separated by long steep ridges 700 to 900 feet high, of red sandstone, stretching, I believe, hundreds of miles from south to north. It will give you an idea of the enormous thickness of these formations when I mention that nearly the entire series of beds are upon their edges, sometimes vertical, and very rarely less than 45°, the inclination being sometimes to and sometimes from the Alleghanies, while all westward of that chain (for many miles beyond the Ohio,) is horizontal, or slightly curving, and comprises the great Central Coal Field of this continent. I have scarcely seen anything in all this vast development of strata under the coal which does not contain the mountain limestone fossils, such as Producta, Spirifers, &c., the former in astonishing profusion, and all having specific differences from the English.

"There is now a project before the Congress at Washington, submitted by the Secretary at War, and closely watched by Mr. Featherstonhaugh; no less than a geological survey of the entire continent, under the government of the United States, to be connected with the Engineer Department. For this purpose, or rather in connexion with it, a Professorship of Geology and Mineralogy is recommended at West Point Military Academy, for the initiation of the cadets, who in time will be dispersed all over the Union, and will carry their industry and zeal into a wide and useful field. Should this plan be adopted, the Professorship will be offered to Mr. F., and my aid has also been asked in cooperation in this grand undertaking. I should be proud to be an associate in such a gigantic project; and here the observations I made previously, with regard to assimilating to your English maps, &c., are applicable, with even greater force*. It will afford a grand opportunity for forming a national museum in the capital.

"I fear there is little chance of discovering formations in this country less ancient than the coal, unless it be in those accumulations in Maryland, containing shells resembling those of the London clay and my old acquaintance the Norfolk crag."

* Mr. Taylor here alludes, we presume, to the propriety of preserving uniformity of colouring between the geological maps constructed in this country and in America.—Edit.

Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London, and by Mr. VEALL, at Boston.

Days of Month. 1834.	Barometer.			Thermometer.			Wind.		Rain.	
	London.		Boston. $8\frac{1}{2}$ A.M.	London.		Bost. $8\frac{1}{2}$ A.M.	Lond.	Bost.	Lond.	Bost.
	Max.	Min.		Max.	Min.					
Oct. 1	30·158	30·126	29·70	66	38	54	E.	calm	...	...
● 2	30·106	30·076	29·65	71	36	56	S.	W.	...	...
3	30·177	30·158	29·72	70	37	52	S.	calm	...	...
4	30·222	30·173	29·69	78	36	55	S.	calm	...	...
5	30·186	30·169	29·62	80	49	58	S.	calm	...	...
6	30·254	30·210	29·67	79	52	57·5	S.	calm	...	...
7	30·217	30·188	29·58	74	50	58	W.	W.	...	...
8	30·168	30.055	29·50	63	56	57	S.	W.	...	...
☽ 9	29·958	29·885	29·37	66	50	59	SW.	W.	...	...
10	30·080	29·990	29·47	60	32	52·5	E.	NW.	...	...
11	30·113	30·087	29·60	65	41	44	E.	NW.	...	...
12	30·150	30·140	29·59	67	40	50	S.	NW.	...	...
13	30·132	29·929	29·55	70	41	57	S.	W.	...	...
14	29·773	29·698	29·25	71	46	56	SW.	calm	0·12	...
15	29·805	29·726	29·30	40	36	48	S.	calm	...	0·23
16	29·727	29·322	29·19	58	42	47	SW.	W.	·04	...
○ 17	29·593	29·313	28·67	58	42	53	W.	NW.	·17	...
18	29·700	29·627	28·97	60	50	44	NW.	NW.	...	...
19	30·046	29·914	29·53	62	53	42	SW.	calm	·02	...
20	29·854	29·447	29·17	61	49	55	S.	W.	·04	...
21	30·271	29·982	29·47	55	36	50	NE.	N.	...	·15
22	29·961	29·797	29·21	56	51	52	W.	NW.	·01	...
23	29·731	29·582	29·	56	33	51·5	W.	NW.	·03	·14
24	29·948	29·796	29·31	49	38	36	NW.	N.	...	...
☾ 25	30·239	30·123	29·65	50	35	41·5	NW.	NW.	...	·15
26	30·496	30·395	29·90	54	45	39	N.	NW.	...	...
27	30·461	30·436	29·90	50	32	49	N.	calm	...	...
28	30·574	30·500	30·	53	44	50	NE.	calm	...	...
29	30·674	30·615	30·10	54	44	45	S.	calm	...	...
30	30·513	30·320	29·80	55	40	48	SW.	NW.	...	...
31	30·244	30·180	29·55	59	48	53	W.	W.	...	...
	30·674	29·313	29·50	80	32	50·6			0·43	0·67

Remarks.

London.—Oct. 1. Hazy: fine. 2. Dense fog: very fine: foggy. 3—6. Dense fog in the mornings: very fine. 7, 8. Very fine. 9—11. Cloudy: very fine. 12. Slight fog: fine. 13. Foggy: very fine. 14. Overcast: slight shower. 15. Fine. 16. Overcast: rain: clear and windy at night. 17. Clear and windy: rain. 18. Clear and windy. 19. Slight fog: drizzly. 20. Fine: rain. 21. Cloudy and cold. 22. Cloudy and windy. 23. Stormy with rain. 24, 25. Clear, cold, and dry. 26. Fine, but cold. 27. Hazy. 28. Foggy. 29. Dry haze. 30. Hazy: fine: foggy. 31. Fine. The weather in September was noticed as having been particularly fine; in this month, the continuation of such renders it still more remarkable. The temperature was often high for this period of the season; on the 5th the thermometer stood at 80° in the shade.

Boston.—October 1. Cloudy. 2—8. Fine. 9. Cloudy. 10—13. Fine. 14. Cloudy: rain P.M. 15, 16. Cloudy. 17, 18. Stormy. 19. Cloudy. 20. Cloudy: rain A.M. and P.M. 21, 22. Stormy. 23. Stormy: rain early A.M. 24. Stormy: ice this morning: rain P.M. 25. Stormy. 26. Fine. 27, 28. Cloudy. 29. Fine. 30. Cloudy. 31. Stormy.

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